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CONCEPTIONS OF FATIGUE AND ADAPTATION[1]

BY CHARLES S. MYERS

National Institute of Industrial Psychology, London, England

Until relatively recent times there were two main conceptions held as to the nature of physiological 'fatigue.' In the first place, it was supposed that since living substance or protoplasm is continually engaged in katabolic and anabolic activity,—that is in breaking down and in building up,—'fatigue' arises when such katabolic processes have become relatively excessive, so that the normal efficiency, correlated with the constant wear and repair of protoplasm, is no longer possible. In the second place, it was believed that 'fatigue' arises through autotoxic action, when the products of protoplasmic activity are not taken away from the active tissues with sufficient speed and completeness.

We have still good reason to retain the idea that the mechanism of living tissue, or, at all events, of muscular tissue, is liable to be clogged, as it were, by the products of its own activity when unremoved. But, especially as the result of the study of glandular secretion and of striated muscular contraction,—we no longer accept in its original form the first mentioned idea of the nature of fatigue. We now have reason to believe that the cells of living tissue manufacture within themselves certain substances—'intermediate' substances, as they may be conveniently called,—which are retained within the cell protoplasm for the purpose of being consumed during or immediately after the functional

[1] This paper was read at a joint meeting of the Sections of Physiology and Psychology at the Toronto meeting of the British Association, August 1924.

activity of the tissue. Thus when a gland secretes, the material, previously prepared by the gland cell, undergoes further change and passes into the duct of the gland as the latter's secretion. After a striated muscle fibre contracts, the material made by and stored up within the fibre—apparently in the form of glycogen—similarly undergoes change, exploding, so to speak, into simpler chemical products, carbon dioxide, lactic acid, etc., the energy derived from such reaction being associated with the production of electricity, heat, etc.

That is to say, we no longer believe that fatigue is primarily caused directly by exhaustion of the metabolic activity of living substance, but rather by exhaustion of the material that had been stored up by the living substance for the manifestation or maintenance of its special function. After that material is spent the cell still lives, it still builds up and breaks down, but it has no longer at its disposal an adequate store of those intermediate products which by their decomposition allow of, say, further glandular secretion or muscular contraction.

The heat and cold spots of the skin appear to provide another instance of the same kind of activity, requiring the same conception of fatigue. When a heat or cold spot is stimulated, it seems to fire off, as it were, whatever it be that is primarily responsible for the sensation of heat or cold. The explosive substance may well be conceived as stored up by the sensory end-organ and as consumed in its functional activity. For if a hot (or cold) stimulus be immediately reapplied to a heat (or cold) spot after a hot (or cold) sensation has been obtained from it, the spot is, as a rule, no longer functionally active. It has become exhausted; it needs time to recover from previous stimulation—time, we may suppose, to manufacture fresh material for its functional manifestation.

In peripheral nerve fibres, phases of inactivity have also been shown to exist, but here they are extremely brief, and are succeeded by equally brief periods of functional activity. The nervous impulse as it travels along a nerve fibre as the

consequence of a continued stimulus is thus really a series of momentary discontinuous changes. It is now generally supposed that each such momentary development of functional activity at any one point of a nerve fibre must involve a movement of ions. Apparently when that ionic movement is completed, time is required before a further movement is possible, and during that time the passage of the impulse is blocked. The cause of this blockage of the impulse—this 'refractory period'—is obscure. We do not know whether it is due to a lack of material available for further ionic movement, or to the need for a reversal of the preceding movement before fresh functional activity is possible, or to some other cause.

The term 'refractory period' generally implies only a *relative* refractoriness and is usually conceived as something very different from fatigue. Thus Sherrington defines it as 'a state during which, *apart from fatigue*, the mechanism shows less than its full excitability.' [1] The reflexes show such refractory periods, *e.g.* the eyelid-reflex. In certain reflexes, *e.g.* in the scratch reflex, the refractory phase is most essential, for if in such a reflex the impulses followed too quickly, its rhythmical, clonic character would be converted into a continuous, tonic one. Hence, if the succession of stimuli here exceed a certain rate, they fail to pass: a refractory phase, a state of block, regularly succeeds each scratching twitch. We have reason to believe that the seat of this refractory phase, so necessary for the performance of reflexes "concerned with cyclic actions occurring in rhythmic series," [2] lies in or near the synapses of the cells of the spinal cord.

It may seem a mistake to apply the same term, 'refractory period,' to conditions which regulate the rate of flow of nervous impulse both at the synapses and along peripheral nerves, or even to consider this phenomenon alongside of those conditions in which cessation of activity is due to the sheer incapacity that follows excessive activity. But in our present ignorance, it is not inconceivable that both the

[1] Sherrington, C., The Integrative Action of the Nervous System, 1906, p. 45. The italics are mine.

[2] *Ibid.*, p. 65.

'refractory phase'—characterised, whether at the synapses or along the nerve fibre, by momentarily recurring blocks—and the 'fatigue state' due to exhaustion of utilisable material will prove to be phenomena of fundamentally the same nature. For where the refractory phase occurs, there fatigue, as ordinarily understood, is absent. Nerve fibres are virtually indefatigable, and the central 'fatigue' occurring in reflexes, is probably something quite different [1] from the fatigue we have so far been studying.

The peripheral nerve fibres, the striated muscle fibres, the heat and cold spots—all resemble one another in showing what is called the 'all-or-none' reaction. Different fibres or spots may, and no doubt do, vary in sensitivity; but if they respond at all, there is little or no gradation in the response according to the strength of the stimulus. The reaction is therefore approximately describable as 'all or nothing'—although probably an absolute all-or-none reaction is never realised. This all-or-none feature is readily explained by the conception of a store of substance, or a capacity for a certain amount of movement, available within the cell, and waiting to be, as it were, fired off more or less completely on the trigger-like application of a stimulus of whatever strength, provided that it is strong enough to be effective.

Within the intact organism, at least, the state of complete exhaustion of a striated muscle is, for various reasons, never reached. In the first place, the muscle fibres are protected by the end plates. These, in excessive nervous stimulation, block the stimulus from further reaching the muscle fibres on which they are placed. Consequently, a stimulus applied directly to a muscle will cause it to contract when neural stimulation has ceased to be effective. In the second place, certain muscle fibres within a given muscle probably require for their response stimuli of most exceptional strength. Thirdly, several muscles may often be alternatively employed in producing the same movement, with more or less change

[1] For example (i) a weak stimulus may 'fatigue' a reflex more than a strong one; (ii) when the scratch reflex is excited by stimulation of a succession of skin points, recovery of the 'fatigued' reflex may occur during continuance of the stimulation, etc. *Ibid.*, pp. 214–223.

of posture, thus temporarily easing certain muscles while the work continues to be performed. Fourthly, under normal conditions there is always maintained a reserve of muscular power, revealed only in states of emotional excitement or nervous 'fatigue,' or under the influence of certain drugs (*e.g.* alcohol), or under suggestion, when the normal higher control of the central nervous system—which by nervous inhibition prevents excessive muscular activity—is reduced. Thus, in the condition of muscular impotence obtained by ergographic experiment, true muscular exhaustion is never realised. A further series of muscular contractions can be elicited if direct stimulation be now applied to the motor nerve. As an intact muscle normally tires, afferent impulses pass up from it to the spinal cord, increasingly inhibiting further volitional activity. When this inhibition fails direction and coördination sooner or later disappear and ultimately disorderly action sets in.

This brings us to consider a totally different form of activity—one devised not for extreme, momentary activity but for relatively moderate, continuous activity. Of this the unstriated muscle surrounding the stomach, intestines, blood vessels, etc., provides an example. Instead of responding by relatively brief outbursts of volitional contraction, we have here a tissue adapted for long-continued, tonic, involuntary contraction. Instead of readily succumbing to fatigue, it is relatively indefatigable.

In the 'intact' organism, *i.e.* where the normal connection with the central nervous system is preserved, this second form of activity is also observable in striated muscle, in addition to the form of activity which we have been previously considering, and which alone remains in the muscle-nerve preparation. Our intact voluntary muscles are continuously in a state of 'tone,' one of firmness or flabbiness, which, though variable, seems almost indefatigable. There are also certain 'postures' which appear to be nearly as insusceptible to fatigue. We carry our head erect, our eyelids opened, our jaws closed for many hours on end; a horse will stand for hours, or even months, without manifesting local fatigue.

XUM

The region appears able to adopt a certain 'set,' a certain 'poise' or 'attitude,' which involves contraction, and which can be uninterruptedly maintained for very long periods. In the case of striated muscle this phenomenon seems to be dependent on a certain coördination and integration, within the central nervous system, of two opposing nervous mechanisms, one concerned with flexion, the other with extension, between which reciprocal inhibition or facilitation is possible.

Within the sphere of the senses, a similar integration occurs between warmth and coolness, corresponding to a range of stimuli between 25° and 38° C.; it also occurs between the opposite sensations of red and green, blue and yellow, etc. Just as the act of flexion involves inhibition of antagonistic extension, so we may, I think, reasonably suppose that sensations of moderate temperature or of colour involve inhibition of the respective antagonistic thermal or colour mechanism. Further, just as a long-continued muscular *posture* may be assumed, in which both flexion and extension are simultaneously active, so, I suggest, a neutral position of sensory *adaptation*, a stable posture, may be reached in which warmth and coolness, or, say, redness and greenness, balance one another—when no sensation of temperature or of colour arises. As is well known, this state of adaptation occurs when we have placed a hand in warm or cool water for some minutes, or when we continue carefully to regard a coloured surface, or wear coloured spectacles long enough. The temperature or the colour gradually disappears; we have become 'adapted,' as we say, to the temperature or the colour stimulus.

The word 'fatigue' is not uncommonly applied to this condition of 'adaptation,' and the two are frequently confused, in total disregard of the wide difference which separates this state of 'epicritic' adaptation from the state of 'protopathic' exhaustion such as is exemplified in the reaction of the heat and cold spots. The latter belongs to a far simpler system. So far as we know, no central nervous connexion exists—at low levels, at least,—between the mechanisms of the heat and cold spots. They react independently, ex-

plosively, and show next to nothing of that gradation of response and that interrelation which occurs in the spotless, diffusely spread sensibility to warmth and coolness. Why warm water comes no longer to be felt as warm, why a red patch comes no longer to be seen as red, is surely to be explained, not by 'fatigue' in the warm or in the red apparatus, but by a relatively or absolutely increasing activity in the antagonistic cool or green apparatus respectively.

Let us therefore be careful no longer to apply the term 'fatigue' to the state of absence of thermal sensation reached when the hand is kept in warm water, or to the state of absence of colour sensation when, say, a yellow patch is carefully fixated. That the condition is not one of ordinary fatigue may of course be readily demonstrated by now exposing the 'adapted' hand to water of indifferent temperature, or the 'adapted' eye to a colourless surface. The water instead of appearing neither warm nor cool, evokes a cool sensation; the surface instead of appearing colourless, evokes a blue sensation. In other words, instead of reaching a state of *fatigue*, we have reached one of *adaptation*, followed (in ordinary conditions) upon the removal of the stimulus by one of *contrast*. Through adaptation the tip of the ear feels no colder than the inside of the mouth, though their actual temperatures may differ by eight or more degrees centigrade. Through adaptation we no longer feel the presence of clothes or eye-glasses. Through contrast we feel the removal of a tooth.

In the colourless series of sensation, adaptation and contrast also occur, though adaptation manifests itself somewhat differently. In colour adaptation, colour sensation disappears, leaving behind some brightness-equivalent in the colourless series of sensations. In brightness adaptation, the colourless stimulus remains, though weaker or stronger than before. A state of equilibrium is reached, one of 'set' or 'posture,' whereby we are able to endure the continuous light of a northern summer without 'fatigue.'

Whether in the intact organism these two systems—the protopathic, 'all-or-none' system, characterised by fatigue,

XUM

and the epicritic 'graded' system, characterised by adaptation and contrast,—exist absolutely apart, whether they are to be localised peripherally in different regions or centrally at different levels,—whether they represent two mechanisms, fundamentally distinct, not only in distribution (according to Head), but (as Rivers supposed) also in date of acquisition, we cannot say definitely. It is possible that they may turn out to have been always and to be everywhere coexistent, the one or the other 'system' being relatively inappreciable and negligible—yet invariably present—in different regions or levels of the organism. Certainly in nerve fibres and in the heat- and cold-spot systems, the all-or-none character seems to hold relatively rather than absolutely; in unstriated muscle fibres, posture and movement seem to be almost identical, while in striated muscle they are intimately related to one another; and in vision, exhaustion to abnormally intense stimulation, rare and ever 'pathological' though it may be, appears to occur as well as adaptation to weaker stimuli. It may even be that just as the striated muscle can respond in two ways,—either by continuous tone or by momentary excitement,—so, if regarded from a wide enough outlook, the sensory apparatus of the skin is a single entity capable of responding now to adaptation, now to fatigue, according to the strength or duration of the stimulus and according to the effects of the stimulus on the nervous and sensory systems.

However this may be, we have to recognise and to distinguish muscular and sensory *fatigue* due to exhaustion, from muscular and sensory *adaptation* due to a state of equilibrium or balance, as it were, in different postures between two opposing mechanisms. Hering, as we all know, regarded this equilibrium or balance in the sensory sphere as one between the opposite processes of anabolism and katabolism. That is to say, according to him, fatigue and adaptation are ascribable to a similar cause, fatigue occurring when the process of katabolism has progressed so far that the cell cannot break down further, adaptation arising when the gradually decreasing katabolism, evoked by a continuous stimulus, is balanced by the simultaneous increasing process of anabolism.

From Hering's view it is difficult to draw any fundamental distinction between fatigue and adaptation, or to see how the two systems to which they belong are related to one another. For all tissues are characterised by katabolism and anabolism; in all tissues there is a reciprocal relation between the two processes, exercise evoking rest, rest evoking exercise, katabolism encouraging anabolism, anabolism encouraging katabolism. Fatigue and adaptation thus appear, according to Hering, to be distinctions rather of degree than of kind. It is hardly surprising, then, that physiologists have so commonly confused them, *e.g.* describing equally a hot spot as fatigued to heat, and the warm-cool apparatus as fatigued, say, to warmth, heedless of the striking differences in respective after-effect. The only alternative hitherto suggested, so far as I am aware, to Hering's metabolic terminology is one which makes use of reversible chemical action.

According to the view I am here taking, fatigue means *lessened functional activity* and occurs in all regions and at all levels—whereas adaptation means a *new functional activity* and arises at higher levels, in circumstances where the activities of two lower antagonistic elementary activities are already so integrated within each of two double mechanisms that excitation of either such mechanism involves excitation of one and inhibition of the other of the two elementary activities. In muscle, for example, excitation of a flexor reflex, in order to evoke flexion, involves inhibition of the opposite extensor action, and vice versa. But when a state of posture arises, a third and different reflex involving a different integration is evoked whereby both flexor and extensor muscles are simultaneously stimulated—it may be in such a way that different muscle fibres are successively being played upon so that tone or posture of the whole muscle can be maintained unchanged for prolonged periods.

I suggest that a similar new mechanism comes gradually into play when a moderate colour or thermal stimulus is applied to the retina or skin. The first effect of such a red stimulus, for example, is to excite the red apparatus, and, according to my view, to inhibit the green apparatus. But

XUM

as its effect continues, a different mechanism is brought increasingly into play, which tends to establish a state of equilibrium, so that one of the two lower mechanisms is no longer 'doing' and the other 'undoing,' but each is actively balancing the other.

We can visualise my conception better if we imagine, let us say, our elbow-joint completely double-jointed, so that, like a double hinge, it can be extended behind the long axis of the arm as far as it can be flexed in front of it. The red apparatus would correspond, say, to flexion, the green to extension. Innumerable postures in positions of extension behind, or of flexion in front of the long axis, are readily conceivable, as the result of prolonged application of as many different strengths of stimulus.

According to the strength of the inducing stimulus, such posture may be strained or easy. Removal of the stimulus, or its replacement by a 'neutral' stimulus, must at once cause a disturbance of the balance which the higher nervous mechanism has superimposed on the two lower antagonistic mechanisms. And any further change in stimulus strength, when once adaptation is set up, must produce its effect not according to its absolute intensity, but only in relation to the existing setting; a faint red stimulus after strong red adaptation being sensed as green, a lukewarm stimulus after strong warm adaptation being sensed as cool, and so on.

We see now how fundamentally, according to my conception, the process of sensory adaptation differs from that of fatigue. I regard it as the result of a process superimposed by a specific mechanism which brings about a temporary fixed 'posture,' involving synchronous balancing activities in two antagonistic mechanisms. A condition analogous to posture or to tone in antagonistic muscles is reached, which can be maintained for a long period, unaltered so long as the environment remains constant. In sensory adaptation, the process is singularly permanent, provided that the stimulus to which adaptation has occurred continues unchanged. But in both cases adaptation implies a specific higher direction which effects a 'set,' or attitude, or balance, between two opposite 'acts.'

We may term this effect of higher direction an 'intrinsic' set. But we have also in passing to recognise another important effect of higher direction, manifest as an 'extrinsic' set, viz.: (a) by inhibition of any extraneous act (or set) unfavourable to the appearance or maintenance of the desirable act (or set), and (b) by facilitation of any favourable extraneous act or set.

Summarising our general results, we have come to distinguish between two main forms of activity—the one involving the consumption of substance intracellularly laid down for outbursts of relatively intense, spasmodic activity, the other, of whose nature we are wholly ignorant, demanding a special direction and resulting in an enduring 'set' of the organism. The outcome of the former we may regard broadly as 'act,' the outcome of the latter as 'attitude.' Both must involve work and the expenditure and degradation of energy, but we may regard the 'act' as characteristic rather of *mechanism*, and as resulting preëminently in the *degradation* of energy, while the attitude is characteristic rather of *life*, and is directed rather towards the *elevation* of energy.

This distinction seems applicable not only to muscular and sensory acts and attitudes but also to the affective consciousness and to the higher mental levels as well. Thus affective tone, with its antagonistic, graded, adaptable, and contrasting pairs, 'pleasure' and 'displeasure,' and certain other similar pairs, provide illustrations of what I have just termed the intrinsic set.

Further, when we come to consider intellectual activity, we find it easy to distinguish the fatigable, explosive 'acts'—of revival, recognition, discrimination, judgment, etc.—from the more sustained extrinsic 'set.' The latter are directed towards preserving a favourable field in which a particular series of interrelated acts can most advantageously be executed. We can, in general, distinguish *acts* of attention, easily fatigable, and dependent ultimately on volition or on some passing stimulus, from *attitudes* of attention, maintainable, especially with appropriate affect, throughout a

XUM

relatively considerable length of time. So long, for example, as the affect of *interest* is present, the attitude of attention readily continues unaltered. As interest wanes, monotony enters; and the involuntarily maintained *attitude* of attention has to be replaced by successive volitional *acts* of attention, (comparable to static muscular contraction), serious fatigue of which is saved by the inhibition associated with the increasing *boredom* which now sets in. A favourable attitude thus goes with a favourable affect; the right 'set' being seemingly associated therefore with the activity especially of the autonomic nervous system. So it is, very clearly, with the protracted activity in unstriated muscular tissue; so it may turn out to be, as has been repeatedly conjectured, in the case of striated muscle,—the medullated nerve fibres being perhaps concerned in effecting relatively brief outbursts of powerful activity, the non-medullated fibres aiding in the maintenance of tone and posture.

Fatigue of voluntary acts of attention appears to be general in character; it diminishes the ability of the self to attend voluntarily in whatever direction. The waning of interest, on the other hand, only weakens the attitude of attention in one specific pose, one specific constellation; the effect of monotony and boredom, is local, not general. Another field of occupation may immediately become interesting, whereupon a new attitude of attention may be readily and successfully adopted. In the one case, what fatigue arises is mainly fatigue of action; in the other, it is something different, it is fatigue of setting of direction.

Action (in the sense in which I am using it here as the momentary execution of a simple 'act') takes place most easily when direction is working favourably, just as movements, in order to be efficient, demand efficient posture. If action be continued after direction is upset, disorder—whether it be in thought or movement—inevitably supervenes.

Interest—whether it be immediate or remote, whether it be innate or acquired,—is essential for successful occupation. Even acts which have become habitual, for which no mental effort is needed, cannot be performed for long if all interest

is lacking, whatever its direction. As is well known, the employee engaged in repetitive work safeguards himself against boredom by recourse to the phantasies of day-dreaming.

The failure of interest during prolonged attention is, as we have seen, accompanied by boredom. If it be allowed still further to increase by further voluntary attention, worry and anxiety result. Conflicting ideas, hitherto repressed because of their incompatible or undesirable emotional concomitants, break loose from inhibition, causing anxious moods, disorderly unreasoned fears, and the like, to intrude into consciousness. Such seems to be the connexion between fatigue and psychasthenia.

A 'set' occurs not only in the case of simultaneous actions; it may occur also in consecutive actions. Besides enduring muscular posture we have to consider enduring muscular rhythm—extension and flexion regularly following one another, or even a whole range of movements recurring, in uniform cycle. The advantages of good rhythm are not less obvious than those of good posture. When the 'eight' of a boat becomes 'ragged,' or when in industry the individual worker loses his rhythm, the strokes become more powerful and more frequent, efficiency is enormously reduced, and output is diminished despite extravagant effort.

The part played by adaptation to rhythm is seen in the well-known experiment of practice in lifting alternate light and heavy weights. A certain set is acquired. This set is comparable to the association of two syllables which are learned together. The two presentations become integrated into a higher, single unit, in consequence of which if the one is given the next tends to appear. So when, after practice in lifting alternate light and heavy canisters of equal size, the light weight is presented, a strong tendency arises, as a result of the previous rhythmical practice, to lift the next weight with more force. If it be no longer actually heavier, it appears, as is well known, to be lighter, because it rises more rapidly owing to the more powerful lift. Fundamentally, the processes of learning, habit formation, and the

XUM

acquisition of rhythm are of the same nature, different simpler units being integrated to form higher more complex ones. They demand on a higher plane direction and control.

Direction and control involve both facilitation and inhibition. Each of these, on the higher planes at least, demands work. I cannot therefore regard inhibition in general as the simple negation or immediate consequence of the excitation of opposite activities. Higher control does not merely consist in the passive drainage of a fixed fund of energy from inhibited to excited regions. Work is done not less in inhibiting one activity than in evoking another. To block any process is as strenuous as to unblock another; a poison may easily convert an inhibiting into an exciting stimulus. The failure of direction and control, the loss of skill and training is reflected in disorder of inhibition as in disorder of excitation.

Most industrial fatigue is predominantly due to fatigue of higher direction and control. As Muscio has shown, efficiency of direction and control varies at different hours of the day, apart altogether from the extent to which it is being exercised. A definite curve of efficiency throughout the day is established in each worker, dependent no doubt on heredity and on past experience. In the factory it is also influenced by the nature of the work, and probably by tradition and by mass-suggestion. The ergograph or dynamometer cannot serve as a satisfactory index of industrial fatigue. For here direction and control are reduced to a minimum, the task is absolutely uniform, and the subject of ergographic or dynamometric experiment is required to 'act' at his very utmost for a relatively brief period. In industrial conditions, on the other hand, direction and control are all important; the worker adapts his effort according to the length of the day and the severity of his work. In ergographic experiment, a state of fatigue may, by reducing higher control, yield a temporarily increased output of work. In everyday life, on the other hand, as Muscio has shown, the curve of felt fatigue throughout the day closely follows the curve of output. Although fatigue of higher 'attitude' may temporarily increase the efficiency of lower activities (as demanded

in tests of muscular power), it involves, as we have already observed, disturbance of that delicate coördination and rhythm which are all important in everyday work and become immediately manifest in wasteful energy and in deteriorated quantity and quality of output.

The ergographic fatigue curve is one of uninterrupted descent. The nearest approach to this in the hourly work curve under industrial conditions occurs when heavy unskilled muscular work is being performed. Rhythmical, skilled or dexterous work, on the other hand, shows improvement during the first hours of a spell of work, as the workers 'warm up,' 'settle down' and become 'adapted' to working conditions. The present study of different forms of work curve according to the worker and the nature of his work may bear useful fruit in the future. It may also help to differentiate the inefficiency due to the effects of fatigue upon 'acts' from the inefficiency due to loss of the higher direction and control of 'attitudes.'

General Summary

Fatigue may arise from the exhaustion of the material stored up by living substance for the manifestation of its specific activity (contraction, heat sensation, etc.) or from the accumulation of the waste products of such activity. This kind of fatigue occurs usually after powerful *spasmodic acts;* and—although these normally require direction or control—it is, at present at least, to be distinguished from the fatigue affecting the milder *continuous set* of control or direction, which results in attitude, posture, rhythm, and orderliness of acts (mental or muscular). Two kinds of 'set' may be recognised as useful. 'Extrinsic set' involves inhibition of unfavourable or incompatible acts or sets; it preserves a favourable attitude. 'Intrinsic set' involves poise between antagonistic acts; in muscle it is manifest as protracted tone or posture, and in visual and thermal sensibility as adaptation to colour (or brightness) and temperature. Visual and thermal adaptation is thus to be regarded as an active state of posture, not as a state of fatigue.

XUM

Throughout mental and muscular activity, the exercise of *direction* and the elaboration of material for the production of *acts* concur—attitude and posture being the setting or matrix in which acts take place. In everyday life, fatigue of direction is of far greater importance than fatigue of act. On it depend the acquisition, preservation, and manifestation of skill, attitudes of attention, etc.; but of its nature we know nothing.

A FUNCTIONAL VIEW OF CONSONANCE [1]

BY JOSEPH PETERSON

George Peabody College for Teachers

Consonance cannot be regarded today as a simple phenomenon, and there seem to be no adequate grounds for calling the experience of consonance an 'elemental sense.' Careful analysis of the conditions under which we experience tones as consonant and a few thoughtfully arranged experiments on the judgments of consonance by different persons under varying conditions will suffice to convince any unbiased person that several factors, varying in relative importance in different circumstances, contribute to the experience that has been described as consonance. The Seashore test of the so-called *sense* of consonance is based on the idea that the subject reacts to a complexity of factors.

The conception of Descartes, Leibnitz, and the Swiss mathematician, Euler, ascribing consonance to some sort of appreciation of vibration ratio, is much too simple and it really explains nothing at all. Moreover, the relationship between degree of consonance and simplicity of ratio in the vibration frequencies is not as direct and simple as this view has maintained, for slightly mistuned consonances remain consonant though their ratios become very complex. In support of their position Lipps has emphasized the view that the higher the degree of the 'common rhythm' the greater the consonance. Thus 2 : 3 has a common rhythm of 1, which is relatively very high so that coincidence of phase in the two series occurs with every second vibration of the one and with every third of the other, whereas the ratio 8 : 11, for instance, has a relatively low common or 'ground' rhythm so that coincidence of phase occurs only every eighth vibration of the one and every eleventh vibration of the other series.

[1] Read at the Baltimore meeting of the Southern Society for the Study of Philosophy and Psychology, April 23, 1924.

XUM

In the former case, the interval between successive coincidences is simple and easy to span, whereas in the latter case it is complex and hard to span. The rhythms are not consciously perceived, but their effect is registered in subconscious processes, and colors the perception of consonance so that 2 : 3 is consonant and 8 : 11 is dissonant. This idea of the complexity of the waves between two coincidences of phase was held by Euler, and that of the subconscious registering of the effect, by Leibnitz. These theories assume the very thing to be explained and so are of little value, and they must assume some contradictory supplementary principle to account for the consonance of slightly mistuned consonances, which have very complex ratios.[1]

The first serious attempt at a really scientific explanation of consonance was made by Helmholtz, though the view was not original with him. "When two musical tones are sounded at the same time," he said, "their united sound is generally disturbed by the beats of the upper partials, so that a greater or less part of the whole mass of sound is broken up into pulses of tone, and the joint effect is rough. This relation is

[1] The theory of H. J. Watt, elaborated in a couple of recent books 'Psychology of Sound,' 1917, and 'The Foundations of Music,' 1919, cannot be adequately criticized here. It seems to me not to belong to scientific theories and may be right or wrong without throwing much light on the subject. It is based on a doctrine of the inclusion of the 'volumes' of high tones in those of lower tones, the latter being larger and based on the excitation of a larger area of the basilar membrane. In the octave the upper tone is supposed to have a volume just one half that of the lower tone. But here again we come merely to a dependence on mere ratio of volumes for balance or symmetry. No real reason is given why a one-half or a three-fourths ratio of volumes gives consonance and a ratio of seven-ninths, for example, does not. Besides this, the theory is based on a questionable physiological assumption—that each tone sets into excitation an area of the basilar membrane beginning at the end at the oval window and extending upward a distance varying inversely in each case with the pitch of the tone. A very low tone may set in action the entire basilar membrane. Watt has overlooked the fact that the entire basilar membrane, or even a very considerable part of it, could not thus vibrate together, because the membrane constantly changes its plane as it revolves about the modiolus. Only a small part of it in any region is approximately in the same plane, and could therefore vibrate at once without mutual hindrance of neighboring parts. Moreover, tonal islands and several other such phenomena are inexplicable on such an hypothesis. The Wrightson theory (T. Wrightson, 'An Enquiry into the Analytical Mechanism of the Internal Ear,' 1918, and E. G. Boring and E. B. Titchener, a review in *Amer. J. Psychol.*, 1920, 31, 101 *ff.*) has many difficulties to overcome before it can be seriously considered.

dissonance. But there are certain determinate ratios between pitch numbers, for which this rule suffers an exception, and either no beats at all are found, or at least only such as have so little intensity that they produce no unpleasant disturbance of the united sound. These exceptional cases are called consonances."[1] For him tones that 'go on sounding uniformly without disturbance' and that have coincident partials are consonant, but "Just as the coincidences of the two first upper partials led us to the natural consonances of the octave and the fifth, the coincidences of higher upper partials would lead us to a further series of natural consonances. But it must be remarked that in the same proportion that these higher upper partials become weaker, the less perceptible become the beats by which the imperfect are distinguished from the perfect intervals" (p. 181, 182), and so no sharp line of demarcation can be drawn. "The transition can be effected very gradually, and observed in all its stages, and hence it is apparent to the simplest natural observation that the essence of dissonance consists merely in very rapid beats. The nerves of hearing feel these rapid beats as *rough* and unpleasant, because every intermittent excitement of any nervous apparatus affects us more powerfully than one that lasts unaltered. With this there is possibly associated a psychological cause. The individual pulses of tone in a dissonant combination give us certainly the same impression of separate pulses as slow beats, although we are unable to recognize them separately and count them; hence they form a *tangled* mass of tone, which cannot be analyzed into its constituents. The cause of the unpleasantness of dissonance we attribute to this *roughness* and *entanglement*. The meaning of this distinction may be thus briefly stated: *Consonance is a continuous, dissonance an intermittent sensation of tone*. Two consonant tones flow on quietly side by side in an undisturbed stream; dissonant tones cut one another up into separate pulses of tone" (p. 226).

Combination tones may beat and cause dissonance when no partials are present. To illustrations and discussions of

[1] 'Sensations of Tone,' 4th ed. (trans. by Ellis), 1877, p. 194.

XUM

this a whole chapter is devoted, and we are told that "Combination tones are the most general cause of beats. They are the sole cause of beats of simple tones which lie as much as, or more than, a minor third apart" (p. 204). Thus Helmholtz gives a sensation basis to consonance and dissonance, but, contrary to the statements or implications of many current writers, he has not based the beating of wide intervals entirely on interference of upper partials, but has involved, in even a more general way, the beats of difference tones. However, since the number of beats of difference tones and of upper partials is always equal whenever both beat series are present, he paid little attention to the former in such cases. Though he undoubtedly saw the similarity between the unity of consonant tones and that of the partials of a clang, he did not emphasize 'synthetic perception' in the former case as he did in the latter, in his controversy with Seebeck (*ibid.*, 62 *f.*).

Stumpf and others have criticized Helmholtz's account of consonance as being negative merely, or as giving an explanation at best only of dissonance, consonance being merely the absence of dissonance. While this criticism is not wholly just—for Helmholtz mentioned 'coincident partials' as well as absence of beats as a condition of consonance —there is a large element of justice in it; but the positive aspect of the theory has of late been strengthened by Krueger. A second point urged by these critics is that two dissonant tones conducted separately one to each ear, so that they cannot beat, are still perceived as dissonant, whereas, they contend, such tones should on Helmholtz's theory be consonant. This criticism has not been supported by the experimental evidence necessary to establish its validity. In the first place, it appears that there is a real difference between such dissonance and that of beats, and an experiment on this sort of beatless dissonance should be carried out on persons unacquainted with or little accustomed to our musical scale, to ascertain the effects of habituation. In the second place, the contention that tones of a dissonant interval so conducted to one's ears should be consonant is

somewhat far-fetched. We must consider certain implications which were not explicitly stated by Helmholtz. We need a careful comparative study of monaural with binaural dissonance.

Helmholtz's discussion [1] of the effect on consonance of the *timbre* of the tones seems to have been overlooked by his critics [2] of the Stumpf school, who deny such effects and who have been strangely neglectful of the evidence of it.

Stumpf has defined [3] tonal fusion as a sort of elemental unity which may, but does not necessarily, preserve the several elements; and this fusion is the essence of consonance for him. This fusion, he has contended, is independent of the absolute pitch, of the relative intensities, and of the *timbre* of the primaries of the interval. These contentions have been taken uncritically by Stumpf's followers and have never received the careful experimental test that they need. My own experiments, by no means adequate on these points to establish a thoroughly reliable conclusion, but carried out under various conditions and with different kinds of primary tones, have rather constantly revealed differences in consonance with variations in one or more of these factors. On the positive side, Stumpf has had very little to contribute to the explanation of the differences in the consonance of different intervals. 'Specific synergy' is only a name, not an explanation at all, and it implies a simplicity in consonance and dissonance and an elemental sensory nature of them that does not find support in the facts of recent experiments.

Ogden's contention,[4] that consonance is really a matter of perception involving a complexity of factors, and that habit plays a very great rôle in determining what intervals for any person are consonant, is well made. The experiments of Meyer [5] and of Moore [6] certainly justify this general view.

[1] Helmholtz, *op. cit.*, 209*f.*

[2] See, for example, Moore, T. H., 'The Genetic Aspects of Consonance and Dissonance,' PSYCHOL. MONOG., 1914, 17, No. 2, p. 6.

[3] Stumpf, C., 'Tonpsychologie,' 127*ff.*

[4] Ogden, R. M., 'Hearing,' 1924, 140*ff.*

[5] Meyer, Max, 'Experimental Studies in the Psychology of Music,' *Amer. J. Psychol.*, 1903, 14, 207–214.

[6] Moore, H. T., *op. cit.*

XUM

But Ogden, regarding the experience of the individual alone as inadequate and too short to effect the changes that have taken place in the perceptions of consonance, has brought in the effects of racial experience in a manner that is certainly questionable. "We have every reason," he says, "for supposing that congenital associations result from racial experience of a definite and oft-recurring sort, to produce instinctive modes of adjustment and attendant complexes of experience which are fundamentally unified." [1] "The nervous correlate of a consonant relationship I regard as nothing other than a relatively simple and economic activity on the part of the sense-organ and nervous system. Such dispositions, I take it, are in the nature of fundamental capacities resulting from very frequent racial experiences" (p. 300). His view is that the fusing intervals are 'the very ones that are most conspicuous among the partials of a clang.' [2] Thus in the clang 1, 2, 3, 4, 5, 6, 7, 8, 9, 10, 11, 12, 13, 14, 15, 16, where we have sixteen partials, the octave (1 : 2) occurs eight times; the fifth (2 : 3), five and the fourth (3 : 4), four times; the major third (4 : 5) and major sixth (3 : 5), three times each; the minor third (5 : 6) and the minor sixth (5 : 8), twice; and no dissonant interval occurs more than once. Now, asks Ogden, "apart from individual habit, which, of course, would vary greatly with experience, does not the frequent occurrence of the most striking fusions—the octave, the fourth, and the fifth—among partial tones, warrant the assumption that adjustment of the ear to frequent combinations of tone can, in some measure, be selected through inheritance" (p. 142)? The answer is evidently, No; this assumption violates all known facts of inheritance, *and it is unnecessary.* We needn't argue the first point. Instead of having 'every reason' for holding to such transmission in heredity of the associations formed in individual experience, we have actually *no evidence for it at all.* Or, if we have, what is the evidence? We must approach the interesting and suggestive fact of the frequency of the

[1] Ogden, R. M., 'A Contribution to the Theory of Tonal Consonance,' PSYCHOL. BULL., 1909, 6, 299.

[2] 'Hearing,' p. 141.

consonant intervals in the partials of a clang from another angle, as we shall presently see.

In my judgment, the work of Krueger [1] points in the right direction for a solution of the consonance problem. Building on that of Helmholtz, he has done a great deal of work on the effect of combination tones, or specifically on difference tones, but he has not gone far enough in the consideration of what he terms 'associative factors.' Strangely enough, he gives little attention to Helmholtz's treatment of difference tones in connection with consonance, the principle on which he himself builds. When the primaries are too far apart to interfere directly, Krueger traces the beats to difference tones. Of these tones he stresses particularly the 'characteristic difference tone' whose ratio always corresponds to the number 1. In pure consonances this tone is really a coincidence of two or more difference tones, or of difference tones and the lower primary, as in the octave. A slight divergence from perfect consonance results in divergence of the several tones coincident at the characteristic tone, and consequent interferences among them, experienced as beats and roughness. For example, the interval 200 : 300 has two, three, or perhaps even four difference tones of 100 v.d. each. Now if the interval is slightly mistuned, say to 200 : 307, the difference tones become, according to Krueger, 107, 93, 14, 79, and 65, whereas they were for the perfect consonance 100, 100, 0, 100, 100. The several difference tones in close proximity will then produce a roughness which destroys the consonance. In the case of the fourth, say 300 : 400, the difference tones would be 100, 200, 100, 0, and 100, in ascending order. Here again there would be many interferences in case of slight mistunement; and so on for other consonances. Granted the existence of the difference tones, even though inaudible, the dissonance of the considerably mistuned interval readily becomes comprehensible. It can also be accounted for otherwise, as Helmholtz held, if upper partials are present;

[1] Krueger, Felix, 'Die Theorie der Konsonanz,' *Psychol. Studien*, 1906, 1, 305–387; 1907, 2, 205–355; 1909, 4, 201–282; 1910, 5, 279–411; also 'Consonance and Dissonance,' *J. Phil., Psychol., Sc. Method*, 1913, 10, 158–160.

XUM

and we must not overlook the fact that his mathematical calculations, as well as those of other authorities, show that *harmonic* combination tones are produced, as well as difference and summation tones. But why should the simple-ratio intervals, 1 : 2, 2 : 3, etc., be consonant or blend into a unity? Is consonance merely the absence of beats? No. Neither Helmholtz nor Krueger has held this. Helmholtz had, however, very little of a positive nature to offer in his coincidence of partials or difference tones, and even Krueger seems not to have realized fully the possibilities of his basic assumptions. He offers little beyond saying that "In the concrete perception of tone combinations—on account of the relative frequency of the intervals and *their similarity to single musical sounds*—several associative factors coöperate." [1]

Now, Krueger's law of determining difference tones—that of constantly subtracting the lowest from the next lowest tone (*e.g.* 4 : 5 → 1, 3, 2, 1, 0) up to about five such operations—is not exactly correct, though most of the difference tones which he assumes can be shown theoretically to have some sort of existence even if not actually audible.[2] It must not be forgotten that very weak, inaudible tones can by interference produce roughness.[3] The mathematical deductions show that in any case the number of difference tones produced may be considerable, though usually some of them do not actually exist even in strength enough to produce beats.

We shall now add two supplements to the Helmholtz-Krueger theory of consonance, which, it is hoped, will enable this theory, the only really scientific explanation of the whole range of auditory phenomena, to meet the better grounded criticisms of it. These supplements are hardly themselves matters of theory; they are principles based on rather well-established facts.

In the first place, we must recognize that what is habit in one generation becomes not the inheritance of the next but

[1] Krueger, *op. cit.*, last reference, p. 160. (Italics are mine.)

[2] Helmholtz, *op. cit.*, p. 412 *f.*; Peterson, Joseph, 'Combination Tones and Other Related Auditory Phenomena,' PSYCHOL. MONOG., 1908, No. 39, p. 105; Schaefer, Clemens, *Annalen d. Physik*, 1910, 33, 1216–1226.

[3] Peterson, *op. cit.*, 111 *ff.*

part of the environment in a real, objective fashion. Language habits and acquirements, besides being aspects of the behavior of people, become printed literature—books, magazines, etc.—and thus afford stimuli to similar habits in the succeeding generations. The direction of development in the later generations is thus limited in one sense and positively stimulated in another. So in the history of music, if the roughness of certain intervals forced their rejection as elements in the accepted scales, while the smoothness or absence of beats permitted certain others to be used, it is obvious that instrument makers, composers, and printers—to say nothing of the direct social effects of habits—would gradually fix the early beginnings so that only the most radical errors could later be corrected. Thus certain intervals, such as the perfect consonances at first, and the thirds and sixths later, would become fixed, as it were, on our culture as objective products of habits, and succeeding generations would be brought up to hear as a rule only these intervals as appropriate consonances. Many other intervals, as Krueger has pointed out, such as 4 : 7 and 5 : 9, would have seemed suitable now if they had by some circumstance found place in the early scales. They would have been part of our musical environment. Thus the habits of one generation become influential in the behavior and mental life of later generations without operating through heredity. Consider, for instance, present problems regarding the fixity of the languages of certain groups and the difficulties of intercommunication, despite the fact that the new-born infants could learn one language as easily as another if they were only in the proper environment for it. This principle is of importance in social psychology.

Secondly, we must not suppose that the unity of consonance is the result of an association of once discrete elements. Ogden's theory errs in positing such synthesis by association on the basis of frequency, though he shows a different attitude when he says: "Like the innate uniformity of parallel finger-movements, as compared with the independent use of a single finger, which is only gradually ac-

XUM

quired, so the *timbre* of a clang first affects us as a whole." [1] The next two sentences are likewise good,—and equally irrelevant or contrary to his theory. The unity of a consonant interval is not a synthesis by association; it is rather only an aspect of physiological processes which have never been dissociated because they have always occurred simultaneously and have varied together. The so-called tastes of many substances, such as spices, meats, onions, castor oil, and others, are mostly not tastes at all, but, never having been differentiated, are 'perceived synthetically,' to use Helmholtz's own term, with the true taste qualities and with other sensations of contact on the tongue. Helmholtz, unfortunately, applied his conception of synthetic perception only to our experience of the clang, whose partials are a unity if we have never been practiced in discriminating them under conditions of varying concomitancy; but the conception is just as applicable to the matter of consonance as to the clang. Indeed, with the exception of the entrance of beats and roughness in dissonances, the two are very similar, and Helmholtz undoubtedly was aware of the fact. It is true that association will also, in the life of the individual, build up new unities in physiological processes, as the conditioned reflex experiments on salivation show; but so far as the cognitive aspects of consonance are concerned, such syntheses are doubtless very rare if they occur at all.

Now to apply this to the case in hand, let us note that when we hear what we call a tone we normally hear as a unity several partials varying in intensity. The pitch is mainly determined, but not entirely fixed, by the first partial. These partials may be represented as follows, large size numbers standing for great intensity: *1*, 2, 3, 4, 5, 6, 7 . . . . The relative intensity of the several partials will of course vary for different tones, but usually the intensity will decrease with the ascending order of the partials. Now, an octave, say with no partials, will be represented thus, *1*, *2*, since all the difference tones will coincide with the lower primary; and if partials are present the similarity to the clang unity

[1] Ogden, 'Hearing,' p. 149.

will be striking. The fifth, the fourth, and the minor sixth will appear as $_{1}, 2, 3$; $_{1}, _{2}, 3, 4$, and $_{1}, _{2}, _{3}, 4, 5, 6$, respectively, the large numbers in italics representing the primary tones. Thus the same partial tones are found in consonances that exist in a clang, and only their *relative intensities* vary. The characteristic difference tone, 1, has the same foundation position in the intervals that the first partial tone has in the clang. In the clang the first partial gives pitch to the whole tonal complex, and the pitches of the upper partials are unperceived except as they tend to make the 'primary tone' full and complex; thus a strong unifying effect is exerted on the whole tonal complex by the first partial, an effect that is little weakened, and certainly never destroyed, by analysis of the tone. In the case of the consonant interval the difference is only one of degree except in one particular. In this case the characteristic difference tone is so weak that it cannot determine the pitch of the tonal complex. It undoubtedly, however, serves to exert a unifying effect on the entire complexity of tones, to give a unity which, like that of the clang, persists after analysis.

If now we represent in the same way a dissonance, say the minor seventh (9 : 16), we at once see why it should be less unitary than a consonance, even if no beating is present, or noticeable. It would appear thus, according to our scheme:

$$_{1(?)}, 2, 3, _{4(?)}, 5, —, 7, —, 9, —, —, —, —, —, —, 16.$$

It is obvious that conditions are not favorable here for a unitary blending, because the relations of partials in a clang are not closely approached here, many elements being left out, and the most intense elements being very far removed from the characteristic tone, 1, if indeed this tone exists at all. Thus it is evident that the simple-ratio intervals approach very near to the conditions found in a clang, which we regularly perceive synthetically because the partials have always appeared together, if present at all, in a definite relationship to the fundamental.

Even the unity of the clang cannot, of course, be urged too much; for, as Miller [1] has shown, the relative intensities

[1] Miller, D. C., 'The Science of Musical Sounds,' 1916.

XUM

of the partials of tones by different musical instruments vary considerably and often high partials are stronger than lower ones; but just as such variations condition different *timbres*, so also different ratios of tones are perceived as having different degrees of consonance or of dissonance, even with the *timbre* of the tones held constant. Aside from the roughness, different dissonances probably have something comparable to differences of tone quality, and with habituation most dissonances might become tolerated and acceptable in a musical scale if actual roughness is not excessive. Of course the objective factors fixing musical intervals, discussed in foregoing paragraphs, operate powerfully against any such changes; but experiments like that by Moore show the possibilities under suitable conditions, if not the probabilities. On this view all the cases brought up by Stumpf against the Helmholtz-Krueger theory of consonance are easily met, but it is of course obvious that certain intervals of very simple ratio are more naturally unitary and were therefore chosen as *the* consonances in the early music of all ethnic groups. The fact that a very slight mistuning of such intervals does not perceptibly affect their consonance or their acceptability in musical scales, certainly suggests the very great influence of habituation. It would be interesting to determine the effects of such slight mistunement on children of various ages and degrees of musical training (in the broad sense of this term).

Most of us, and certainly most musicians, probably have a tendency greatly to exaggerate the distinctness of the consonance experience. I have found that by substituting a variable fork for the lower *c* in an excellent set of Edelmann forks of just temperament, it is possible to diminish or to augment the regular consonances by considerable amounts before college students, many of them of musical training, would feel any serious unnaturalness in the intervals, especially when, by faint sounding, beats are not aroused. By this means only the tonic is made to vary, and the influence on the different intervals can be conveniently studied. Moreover, preliminary trials with the Galton whistle indicate

that in the case of very high tones, even musically trained persons have but the faintest idea as to what intervals are formed by successive tones. It is hoped that we shall find opportunity in the near future to carry out a carefully controlled experiment of this sort with simultaneous tones. An interesting field of research, one of importance to music as well as to general psychology, lies open here. As yet we know too little to warrant any categorical statements as to the various factors affecting or conditioning consonance. What is needed is more experimentation, more carefully ascertained facts.

Recently in an experiment on the influence of pitch on consonance, I found the following preferences by the paired comparisons method on eleven subjects, all but two of whom had had musical training which in some cases was very extensive. Each pair of intervals was twice presented but in random order, the second presentation being the reverse of the first. For example, if the first time $c^1 : e^1$ was given first when compared with $c^1 : f^1$, the order would be reversed when these intervals again occurred. Experimental precautions as to allowing equal time of presentation and of intervening periods were observed. Gamut bells made of steel bars were used, these being in equal temperament.

Ratio of interval..	4:5	3:4	1:2	3:5	4:5	2:3	2:3	5:6	2:5	3:5
Interval preference.....	$c^1 : e^1 > c^1 : f^1 > c^1 : c^2 > c^1 : a^1 > c^2 : e^2 > c^1 : g^1 > f^1 : c^2 > c^1 : e^{1b} > c^1 : e^2 > e^{1b} : c^2$									
Number of judgments favoring interval..	150	131	130	123	120	118	111	98	85	81

To state the results of this experiment in another way, we found that whenever the same musical intervals were compared but at different pitches, c^1 being the lowest tone used, preferences were expressed as follows:

Interval Ratio	Number Favoring		
	Lower Interval	Higher Interval	Neither Interval
Major third, 4 : 5	8	2	1
Minor third, 5 : 6	6	4	1
Fifth, 2 : 3	6	4	1
Major sixth, 3 : 5	10	1	0

These results, which are regarded as preliminary and in no sense final, indicate that, so far as intervals above middle *c* are concerned, those intervals which have been experienced most often are preferred to similar intervals of which the degree of habituation has been less; or, if it is not safe to assume most experience with intervals nearest *c'*, that, all else equal, the higher the interval the less consonance it has. If it were not hazardous to conclude from so few cases, we might also infer that the less perfect the consonance the more it suffers in depreciation as the pitch rises. These tentative results, at any rate, suggest interesting lines of attack in more thorough-going and extensive experimentation than our time permitted. It should be said that the subjects used in this experiment had studied the various views of consonance and were asked to avoid as far as possible taking account of the pleasantness of the intervals, but the results do not show that they were very successful in this attempt.

Experience of this sort leaves one in grave doubts as to the possibility of getting reactions to the pure fusion (a cognitive quality) of intervals. A few years ago there was a tendency to draw the line sharply between the cognitive and the affective aspects of consonance, but it must be recognized that even this distinction, useful enough in a rough sense and for purely practical purposes, cannot be drawn along strict lines. It is evident now that habituation is so important in our consonance experiences that every judgment or reaction is largely affected by it, and it is not easy to put this element wholly either with cognitive or with affective factors. Looked at from the standpoint of behavior any such distinction tends to vanish, or to become uncertain.

Consonance preferences are considerably influenced, we find, by numerous overlapping attitudes and sets engendered by the general relationships of tones under which the intervals reacted to are presented. Tonic effects, preparations for resolutions, associations with this or that progression in familiar musical selections, etc., are almost inevitable and seem to be troublesome to many subjects. There is hardly such a thing as a purely independent comparison of two sets of intervals on their own degrees of consonance. This aspect of the subject needs careful investigation. In view of these various attitudes developing from different sequences of intervals, it is interesting to compare the successive scores made by identical subjects in the Seashore consonance test which reproduces the immediately objective conditions of the several intervals about as constantly as machinery can do. Even under objectively controlled conditions of this kind various incidental irrelevant stimuli received during the tests, and attitudes carried over from previous moments, are bound to have their effects, thus altering the constancy of a subject's responses to any given pair of intervals.

Recently we have given this test to groups of college students—in Peabody College and Fisk University—for race comparisons. The test was given twice in each institution to obtain reliability coefficients, but as it was given at the assembly hour, and attendance is purely voluntary at Peabody, the number taking both tests here was not as large as we had hoped to get. The first test period at this institution was very successful as to the number of subjects involved, having been well advertised, but the curiosity had worn off when the second test period came round, so that, despite our efforts to have all who took the first test return for the second one, the number present was not large, and many persons who took this test had missed the first one. There is evidence that those who repeated the test in this case were as a group a superior sampling. At any rate the Peabody group improved in the second test over the first more than did the colored group in Fisk University. It is interesting to note, and this is only incidental to our present purpose, that the

XUM

colored students were superior in this test as well as in the pitch discrimination test. Only these two tests have yet been tried. Plans are under way for a much more extensive investigation of race differences in musical abilities. Our present interest in this report is in the reliability coefficients of the consonance test. For the negroes, 197 cases, this is $.52 \pm .03$, while for the whites, 89 cases, it is $.68 \pm .04$. The lesser reliability of the test shown in the case of the colored group is probably due to their greater lack of emotional control in the first test. These students showed a tendency to laugh at extreme dissonances, which tendency could not be completely suppressed even after cautions in the fore exercises and a reminder again before the giving of the test. After a relaxation of this kind it was doubtless impossible to get down to the best attention before the next pair of intervals were played. This interpretation of the lower reliability in the case of the colored group is borne out by the fact that the pitch test also shows less reliability in the case of the colored group ($.88 \pm .02$ for the whites of whom there were 79, and $.77 \pm .014$ for the colored students, 193 in number).

Summary

The theory of consonance which bases its explanation directly on the ratio of the primary vibrations, whether in the form held by Lipps or by Stumpf, makes consonance much more simple than it has actually been found to be, and, moreover, this theory really explains nothing at all. It is contradicted directly by the fact that slightly mistuned consonances, *e.g.* 201 : 300, instead of being highly dissonant, as would be expected, are consonant. The theory also fails to account for the effects on consonances of absolute pitch changes and of variations in *timbre*. Watt's theory is based on a hypothetical balance of tonal volume that cannot be tested. It seems to be immaterial whether the theory in this regard is true or false. Moreover, Watt's assumption of simultaneous vibration of the entire basilar membrane with certain tones is physically absurd, for the different sections of this membrane are in different planes. Tonal islands and

various other phenomena also contradict this theory. The Helmholtzian theory of consonance is the only thoroughgoing scientific theory that has been offered. It is not affected by the question as to whether or not the cochlea is the resonating mechanism. Helmholtz employed both upper partials and difference tones in accounting for the roughness of intervals which are dissonant, and he regarded the latter as the more generally operative factors in the production of beats, a fact that many of his critics have seemingly overlooked. Krueger has added a very good supplement in his suggestion of the important rôle of the characteristic difference tone in imparting unity to the tonal mass and roughness, in connection with conflicting tones, to the dissonances. Helmholtz did not stress sufficiently the effects of habituation. Ogden's harmonic theory errs in making consonance a *synthesis* based on association, and in assuming a transmission in heredity of the effects of the more frequent occurrence of certain intervals than of others. We have suggested that this is biologically erroneous and that the assumption is unnecessary. Habits have effects on physical environment, fixing themselves on musical instruments and on musical literature, and so they limit and give direction to the development of musical practice and appreciation in successive generations. It has also been shown in this paper that habituation is very important in the experience of consonance, and the method of its operation has been indicated. A number of important experimental attacks on the problem of consonance have been suggested, and some preliminary results are given. The present need in relation to this problem is, above all, more reliable facts on various points which have unfortunately been regarded by many writers as settled.

XUM

A STUDY OF INDIVIDUAL DIFFERENCES IN LEARNING [1]

BY MARGARET KINCAID

Smith College

This investigation was undertaken with a view to summarizing and supplementing the evidence regarding the changes which occur in the relative attainments of different individuals in any given partly practiced function as a result of equal additional amounts of practice in that function. This subject has received treatment primarily from writers who argue about the relative importance of 'heredity' and 'environment' in producing individual differences.

In the equipment for behavior possessed by an individual at birth, we may distinguish: first, a native set of instincts, reflexes, etc.; second, varying inherent degrees of ability to learn along various lines; and third, an inheritance of structures outside of the nervous system, such as bones, muscles, glands, etc., and capacities for growth.

The modification of original endowment is affected by a number of environmental factors: *e.g.* number of repetitions of specific functions, distribution of repetitions through a period of time, order of practice for different elements in a total activity, examples available for imitation, and many others.

The question then arises: Are the differences in achievement in a given function which are found among individuals due primarily to differences in original nature in the sense above described, or to varying opportunities and advantages presented by the environment?

Suppose that we give equal amounts of practice to members of a group in a function in which they are already partly practiced, and in which they display differing degrees of skill, thereby diminishing the relative differences in total

[1] From the Psychological Laboratory of the University of Minnesota.

amounts of practice. If we find that the initial differences in degrees of skill have been thereby increased, then we must conclude that these differences were due less to differences in previous amounts or conditions of practice than to native capacities for learning through practice. If, on the other hand, we find that the individuals who are superior at the beginning of our investigation profit less by equal units of practice than do the individuals of initially inferior ability, then our result is largely negative in significance, leaving it unclear which of several factors has had the greater part in determining the differences before the experiment. There may have been differences which arose in inborn reflexes and instincts, and which practice gradually reduced. There may have been differences in previous amounts or conditions of practice. There may have been differences in capacity for rapid learning which held through the early stages of learning but diminished in the later stages.

Thorndike has rather definitely concluded that the first of these two premises is correct. He states: "The facts found are rather startling. Equalizing practice *seems to increase differences*. The superior man seems to have obtained his present superiority by his own nature rather than by superior advantages of the past, since during a period of equal advantages for all, he increases his lead. . . . So far as they go, experiments in practice have given no support to the common assumption that differences in external conditions are responsible *for the bulk of the variation* found among men of the same race and general social status." [1]

Such statements as the preceding are largely responsible for the initiation of this study.

Statement of Problem

First, we must ask ourselves what is implied regarding the relations between individuals in a group by statements that differences between them increase or decrease with practice. Figs. 1–4 represent in simple hypothetical form four possibilities with regard to changes in differences throughout a practice experiment.

[1] 'Educational Psychology,' p. 305, 307.

In Fig. 1 absolute differences remain approximately the same, whereas relative differences diminish. The opposite picture is presented by Fig. 2 where the relative differences remain practically unaltered, while the absolute differences increase. In Fig. 3 both absolute and relative differences

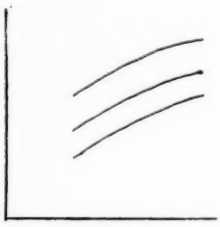

FIG. 1

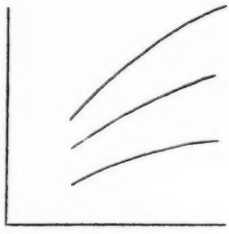

FIG. 2

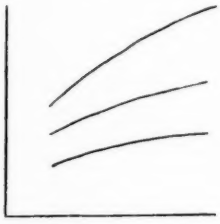

FIG. 3

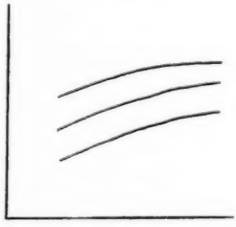

FIG. 4

increase, whereas in Fig. 4 they both diminish. In these figures, + 1 correlations between initial and final positions in achievement have been assumed merely for the sake of simplicity in the diagrams. It is obvious that similar alternatives can be conceived, in which these correlations are low or even negative, and analogous figures to represent these could be constructed.

The restricted character of our problem must be noted here. Suppose that we were able to plan experiments in such functions that subjects could be obtained who had had no practice contributory to the functions to be tested and who could be kept at practice under controlled conditions until the greatest accomplishment of which they were capable had been assuredly obtained. We should obviously then

have a problem of much greater scientific definiteness and comprehensiveness than the problem as to the relative importance of heredity and environment in any learning problem which has actually been experimentally attacked or which practically speaking can be attacked. In the ordinary experimental learning problem, the function tested undoubtedly involves many reactions which have been practiced by our subjects in varying degrees in other connections. Moreover, it is impossible from a practical point of view to continue an investigation until all subjects have reached and are consistently maintaining the greatest accomplishment of which they are capable.

We are not asking in a universal sense the question: Which is the more important factor in learning, hereditary capacity or amount and kind of practice? To this, there could be, of course, only one answer. For all activities, at least, in which we make special adaptations to products of civilization, both are absolutely essential, and the comparison is a comparison of infinities. Suppose, however, that we take the question to mean: Which factor is of more importance in determining the actual differences prevalent under present social conditions? Then we have an experimental problem, with which we can attempt to deal. We must not, of course, assume in advance that our answer will be the same for every type of activity or for every group of subjects. We cannot anticipate a single simple answer to a question as complex as the one which we are proposing.

The present paper is limited to a summary of the conclusions drawn from the main experimental investigations of the past bearing upon our problem, and from two new experiments by the writer carried on under much the same limitations of scope as have characterized former experiments,[1] a statistical assembling and re-analysis of data from all of these sources; and an attempt to draw such inferences as the total experimental evidence justifies on the question of

[1] These investigations are not as yet in print. For the present, full typewritten accounts, including original data, may be obtained by application to the Department of Psychology at the University of Minnesota.

XUM

individual differences in attainment, and the effects upon these differences which additional amounts of practice produce.

Summary of Previous Findings

Table I has been constructed as a summary of various studies from the point of view of the authors' conclusions relative to the effect of practice on the changes in differences in the group. Among the possible causes of the opposing conclusions are the kind of subjects, the length of practice, the unit of measure of practice, the type of problem investigated and the method employed in measuring the changes in differences. These are represented accordingly in columns 2–6 of the table.

It may be observed by inspection of the table that no one of these causes furnishes sufficient explanation of the differing conclusions. It seems clear, therefore, that before a general conclusion can be reached, the learning data must be more thoroughly analyzed, and data from different sources must be more systematically compared: first, in regard to reliability; and second, in regard to the methods of treatment of the results.

Causes rendering data unreliable are realized in theory but neglected in experimental practice. Such, for instance, are the basing of conclusions upon too few cases, and inadequate and varying control of experimental conditions. Greater confidence could be placed in experimental findings if more complete descriptions were given of incentive, instructions, and conditions in general, and if original data were rendered accessible.

Measures of 'initial' and 'final' abilities have been selected arbitrarily without investigation of their reliability. For example, it is obviously unreasonable to consider the first day's work as a measure of an individual's ability, if so many chance factors have been operative that a low correlation would be obtained between the first and the second day's records. Unless there is clear indication that differences in rank order between the first and second day's records are due to differences in rates of learning in the function that

TABLE 1*A*

EXPERIMENTS WHICH APPARENTLY INDICATE THAT DIFFERENCES INCREASE WITH PRACTICE

1 Investigator	2 Subjects	3 Length of Practice	4 Unit of Measure	5 Problem	6 Measure of Changes in Differences
Thorndike '08	33 adults	50–96 examples (5–6 per day)	attainment (corrected)	mental multiplication	Ratios of worse to better records
Starch '11	8 adults	14 daily periods	time	mental multiplication	Gross gain (Thorndike) Cor. between initial ability and gross gain (Chapman '14)
Wells '12	10 adults	30 daily periods	attainment	addition	Curves of gross and relative gains
Kirby '13	1350 children	75 min. 60 min.	attainment	addition division	Curve showing relation between initial ability and improvement (Chapman '14)
Thorndike and Donovan '13	29 school children	30 periods, 2 daily	attainment (corrected)	addition	Gross gain
Thorndike and Hahn '14	129 school children	initial and final test 15 min. 90 min. practice	attainment (corrected)	addition	Gross gain
Chapman '14	22 adults	10 periods on 5 days	attainment	color naming, cancelling 2's, cancelling 3's, opposites, addition, mental multiplication	Gross and per cent. gain. Cor. between initial ability and improvement
Thorndike '16	64 adults	3840 products	attainment	writing products	Gross gain
Thurstone '18	165 adults	100 hrs.	attainment	telegraphy	Standard deviation. Quartile deviation
Thorndike '16	11 adults	10 daily periods, 2 per day	attainment	cancelling 2's, cancelling 3's, addition, mental multiplication	Gross gain
Henmon '20	128 school children	9 periods (1 a month for 9 mos.)	attainment	reading, writing, spelling, arithmetic	Gross gain, group deviation from average

XUM

is being tested, it is more reasonable to combine successive measures until a high degree of correspondence between series of records has been obtained. By so doing we know that our selected amount of practice gives us a reliable measure of abilities, since the addition of a similar amount of practice will not radically alter the rank orders of the group.

Criticisms directed against the various methods of measuring changes in learning have been too fully discussed by Whitley to need more than a comment here. That there is no single measure which covers all of the facts is clear, since various measures applied to the same data give quite opposite results. Nevertheless conclusions in general terms are continually drawn from single measures.

TABLE I*B*

EXPERIMENTS WHICH LEAVE IT QUESTIONABLE WHETHER DIFFERENCES INCREASE OR DECREASE WITH PRACTICE

1 Investigator	2 Subjects	3 Length of Practice	4 Unit of Measure	5 Problem	6 Measure of Changes in Differences
Wimms '07	24 school boys	18 daily periods	attainment	addition, mental multiplication	Correlation between attainment and improvement
Myers '18	27 adults	4 periods, 20 trials per period	attainment	card sorting	M.D. / Av.
	14 adults	23 daily periods	time	fitting colored cubes	
	14 adults	25 daily periods	time	typewriting	
Phillips '19	school children (various nos.)	varying	attainment	math. tests	Percentile gain

ANALYSIS OF STATISTICAL EVIDENCE

It is proposed in this section of the paper to apply a common set of measures to an extended group of learning experiments relevant to our problem, with the hope that if the results of these investigations are put in directly comparable form, it may be possible to make clear just what

TABLE IC

EXPERIMENTS WHICH APPARENTLY INDICATE THAT DIFFERENCES DECREASE WITH PRACTICE

1 Investigator	2 Subjects	3 Length of Practice	4 Unit of Measure	5 Problem	6 Measure of Changes in Differences
Thorndike '10	19 adults	7 daily periods	attainment (corrected)	addition	Correlation between initial ability and per cent improvement
Wells '11	8 adults	22 daily periods	time	free association reactions	Inspection of practice curves
Whitley '11	9 adults	20 daily periods	time errors	lifted weights, maze, mental multiplication, counter sorting, cancelling A's	Correlation between position at start and: 1—position at finish 2—gross gain 3—per cent.gain
Thorndike '16	11 adults	10 daily periods, 4 per day	attainment	typewriting	Gross gain
Murphy '16	30 adults	34 daily periods	attainment	javelin throwing	S.D.
Peterson '17	26 adults	10–50 hours	errors	ball tossing	Error curves (approach each other)
Woodrow '17	20 F.M. children, 16 normal children	13 daily periods	attainment	form-sorting	Correlation between initial ability and gross and per cent improvement
Peterson '18		(Thurstone's data)			$\frac{\text{S.D.}}{\text{Av.}}$, $\frac{\text{S.I.R.}}{\text{Av.}}$
Perrin '19	22 students	10 periods (1 a week for 10 wks.)	time	analogies, mirror reading	Correlation between general excellence and amount of improvement
Lashley '15	19 adults 9 adults	12 shots for 30 days	attainment	archery, rifle practice	Per cent and gross gains

general conclusions they justify. In order to obtain adequate statistical evidence, if possible, twenty-four sets of data have been brought together, and seven methods of computing changes in differences have been applied to them. They

have been obtained from the investigations referred to above and from an article by McCall. McCall's paper is on a subject irrelevant to our purpose, but it presents data well adapted to our uses.[1]

As has been shown above (See, *e.g.* Table I.) the data in certain investigations are expressed in attainment units, whereas in others error or time units have been employed. In the first case, gains are shown by an increasing score, in the second case by a decreasing score. In order that the respective measures obtained might be directly comparable, we have converted error and time scores into accuracy and attainment units by using in such cases the reciprocals of each of the original records in our computations in Section *B* of Table II. This procedure affects Columns 1, 12–15, and 24.

Obviously the reciprocal of a score in time will give us a score in terms of speed; in other words, in attainment per unit of time, something which is comparable to attainment in given equal units of time. Again, the reciprocal of a score in terms of errors will give us a score in terms of accuracy, which is obviously the aspect of the error problem which would be expected to be positively comparable with attainment.

It has been beyond our power to remove all of the causes of the unreliability of the data with which we have dealt. We have had to accept the validity of scores as they stand, and our conclusions will always have to be stated cautiously with an eye toward the extremely small number of cases and of practice periods in many of the studies.

In Section *A* of Table II we have determined coefficients of reliability as described above (See p. 43) in order that we might obtain reliable measures of initial and final ability, or to speak more accurately, of ability near the beginning and near the end of the practice periods.

[1] Of four cancelling letter and cancelling number tests, we have used the one in each group which gives the highest reliability coefficient between successive series of measurements.

XUM

TABLE II

SECTION A. COEFFICIENTS OF RELIABILITY AND CORRELATIONS BETWEEN INITIAL AND FINAL ABILITY

		1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24
		Thorndike '08 Multiplication	Chapman '14 Addition	Chapman '14 Multiplication	Chapman '14 Color Naming	Chapman '14 Opposites	Chapman '14 Cancelling 2's	Chapman '14 Cancelling 3's	Thorndike '16 Cancelling 2's	Thorndike '16 Cancelling 3's	Thorndike '16 Addition	Thorndike '16 Multiplication	Thorndike '10 Addition	Whitley '11 Maze	Whitley '11 Multiplication	Whitley '11 Cancelling A's	Thorndike '16 Typewriting	Woodrow '17 Form Sorting F. M.	Woodrow '17 Form Sorting, Normal	McCall '16 Cancelling 5's	McCall '16 Cancelling 3's	McCall '16 Addition	McCall '16 Copying Addresses	Kincaid Braille Writing	Kincaid Dart Throwing
1	No. of cases	28	22	22	22	22	22	22	11	11	11	11	19	9	9	9	11	20	16	71	71	72	75	31	28
2	Series of periods correlated	1 2	1, 2, 3 4, 5	1, 2, 3 4, 5	1, 2, 3 4, 5	1, 2, 3 4, 5	1, 2, 3 4, 5	1, 2, 3 4, 5	1 2	1 2	1 2	1 2	1 2	1, 2, 3 4, 5	1, 2, 3 4, 5	1, 2, 3 4, 5	1 2	1 2	1 2					1 2	1 2
		8 9	7, 8 9, 10	7, 8 9, 10	7, 8 9, 10	7, 8 9, 10	7, 8 9, 10	7, 8 9, 10	19 20	19 20	19 20	19 20	6 7	15, 16, 17 18, 19, 20	17, 18 19, 20	17, 18 19, 20	39 40	12 13	12 13					46 47	39 40
3	Correlation between first two	.83	.97	.92	.92	.83	.78	.89	.91	.95	.94	.80	.78	.73	.62	.73	.89	.60	.69	.88	.79	.88	.67	.81	.84
4	Correlation between last two	.81	.98	.88	.84	.85	.91	.92	.90	.70	.81	.98	.97	.73	.66	.95	.93	.78	.80	.82	.88	.97	.65	.91	.79
5	Correlation between 2d and next to last	.76	.97	.88	.88	.86	.92	.93	.02	.76	.87	.47	.82	.17	.92	.68	.76	.63	.76	.67	.49	.88	.52	.33	.76
6	Correlation between 1st and last	.68	.96	.87	.87	.59	.75	.85	.20	.63	.59	.38	.71	.15	.50	.77	.92	.29	.64	.51	.33	.79	.60	.28	.44
7	Correlation between 1st and last corrected [1]	.88	.99	.97	.99	.85	.99	.99	.07	.84	.83	.47	.87	.22	1.06	.87	.92	.63	.95	.68	.48	.91	.85	.35	.72
8	Correlation between 1st and next to last	.66	.91	.85	.90	.79	.82	.82	.35	.85	.87	.43	.76	22	.52	.90	.88	.27	.69	.56	.50	.81	.52	.41	.70
9	Correlation between 2d and last	.75	.97	.86	.85	.81	.90	.91	.08	.42	.69	.43	.80	.30	.62	.67	.92	.59	.76	.68	.32	.84	.64	.46	.51
10	Correlation between 1st and last corrected [2]	.85	.97	.94	.99	.95	1.02	.95	.20	.73	.90	.48	.90	.36	.89	.94	.99	.59	.97	.76	.48	.90	.88	.50	.74

[1] By formula True $r = \frac{\sqrt{P_1Q_2 \times P_2Q_1}}{\sqrt{P_1P_2 \times Q_1Q_2}}$.

[2] By formula True $r = \frac{\sqrt{P_1Q_1 \times P_2Q_2}}{\sqrt{P_1P_2 \times Q_1Q_2}}$.

Section B. Measures of Changes in Differences

		Thorndike '08 Multiplication	Chapman '14 Addition	Chapman '14 Multiplication	Chapman '14 Color Naming	Chapman '14 Opposites	Chapman '14 Cancelling 2's	Chapman '14 Cancelling 3's	Thorndike '16 Cancelling 2's	Thorndike '16 Cancelling 3's	Thorndike '16 Addition	Thorndike '16 Multiplication	Thorndike '10 Addition	Whitley '11 Maze	Whitley '11 Multiplication	Whitley '11 Cancelling A's	Thorndike '16 Typewriting	Woodrow '17 Form Sorting F. M.	Woodrow '17 Form Sorting, Normal	McCall '16 Cancelling 8's	McCall '16 Cancelling 3's	McCall '16 Addition	McCall '16 Copying Addresses	Kincaid Braille Writing	Kincaid Dart Throwing
11	Average (beginning)	261	65	43	201	68	91	95	113	128	61	61	159	580	363	205	205	121	122	43	90	31	11	102	193
12	Average (end)	521	84	75	222	117	110	111	173	162	108	189	231	781	698	379	336	175	176	72	137	57	26	543	250
13	S.D. beginning	96	19	14	25+	14	12	11	15	15	16	17	41	172	198	56	84	17	16	22	27	14	4	29	73
14	S.D. end	217	22	21	25−	17	15	16	18	13	23	76	66	193	258	115	60	32	22	15	24	29	6	92	77
15	Greater S.D.	E	E	E	B	E	E	E	E	B	E	E	E	E	E	E	B	E	E	B	B	E	E	E	E
16	$\frac{\text{S.D.}}{\text{Av.}}$ beginning	.37	.29	.33	.13	.21	.13	.12	.13	.12	.26	.28	.26	.30	.55	.27	.41	.14	.13+	.51	.30	.45	.36	.29	.38
17	$\frac{\text{S.D.}}{\text{Av.}}$ end	.42	.26	.28	.11	.15	.14	.14	.10	.08	.21	.40	.29	.25	.37	.30	.18	.18	.13−	.21	.18	.51	.23	.17	.31
18	Greater $\frac{\text{S.D.}}{\text{Av.}}$	E	B	B	B	B	E	E	B	B	B	E	E	B	B	E	B	E	B	B	B	E	B	B	B
19	Ratio $\frac{\text{worst}}{\text{best}}$ beginning	.22	.38	.20	.60	.42	.60	.67	.66	.65	.40	.35	.41	.49	.18	.40	.23	.54	.64	.35	.24	.14	.05	.32	.16
20	Ratio $\frac{\text{worst}}{\text{best}}$ end	.21	.39	.25	.68	.58	.56	.55	.76	.78	.45	.25	.30	.71	.38	.42	.58	.48	.62	.26	.14	.03	.33	.50	.32
21	Greater ratio	B	E	E	E	E	B	B	E	E	E	B	B	E	E	E	E	B	B	B	B	B	E	E	E
22	Ratio $\frac{\text{next worst}}{\text{next best}}$ beginning	.34	.42	.38	.70	.44	.68	.69	.70	.75	.51	.49	.44	.80	.69	.66	.43	.66	.68	.22	.25	.14	.15	.39	.26
23	Ratio $\frac{\text{next worst}}{\text{next best}}$ end	.22	.46	.36	.74	.64	.61	.61	.78	.81	.53	.35	.50	.73	.41	.55	.63	.52	.71	.39	.47	.11	.36	.57	.34
24	Greater ratio	B	E	B	E	E	B	B	E	E	E	B	E	B	B	B	E	B	E	E	E	B	E	E	E
25	Av. highest 25% beginning	397	93	63	189	87	105	109	133	145	82	81	213	734	594	278	327	142	149	59	123	51	16	140	283
26	Av. highest 25% end	703	116	97	245	136	110	129	175	171	126	231	287	929	1043	558	427	195	192	84	148	94	31	607	301
27	Av. lowest 25% beginning	158	42	26	131	48	85	78	95	109	42	38	109	458	205	144	129	97	102	17	61	16	6	70	96
28	Av. lowest 25% end	316	63	52	189	105	92	90	166	152	84	133	162	695	579	294	278	156	153	59	130	31	22	505	188
29	Gross gain highest 25%	306	23	34	56	49	5	20	42	26	44	150	74	195	449	280	100	53	43	25	25	43	15	467	18
30	Gross gain lowest 25%	158	21	26	58	57	7	12	71	43	42	95	53	237	374	150	149	59	51	42	69	15	16	435	92
31	% gain highest 25%	.77	.25	.54	.30	.56	.05	.18	.32	.18	.54	1.85	.35	.27	.76	1.01	.31	.37	.29	.42	.20	.84	.94	3.34	.06
32	% gain lowest 25%	1.00	.50	1.00	.44	1.19	.08	.15	.75	.39	1.00	2.50	.49	.52	1.82	1.04	1.16	.61	.50	2.47	1.13	.94	2.67	6.21	.96
33	Greater gross gain	H	H	H	L	L	L	H	L	L	H	H	H	L	H	H	L	L	L	L	L	H	L	H	L
34	Greater % gain	L	L	L	L	L	L	H	L	L	L	L	L	L	L	L	L	L	L	L	L	L	L	L	L
35	Correlation between initial ability and gross gain	.17	.34	.41	−.14	−.19	.05	.30	−.62	−.09	.27	.20	.05	−.23	.18	.48	−.42	−.13	−.11	−.69	−.57	.57	−.10	.10	−.46
36	P.E. of correlation between initial ability and gross gain	.12	.14	.13	.15	.14	.15	.14	.10	.15	.14	.14	.15	.14	.14	.11	.13	.15	.15	.04	.05	.05	.05	.12	.09
37	Correlation between initial ability and per cent gain	−.26	−.43	−.38	−.36	−.55	−.24	.15	−.89	−.58	−.56	−.18	−.22	−.42	−.33	−.27	−.79	−.36	−.40	−.55	−.81	.27	−.63	−.89	−.60

Section C. Summary of Indications as to Increase or Decrease of Differences

		Thorndike '08 Multiplication	Chapman '14 Addition	Chapman '14 Multiplication	Chapman '14 Color Naming	Chapman '14 Opposites	Chapman '14 Cancelling 2's	Chapman '14 Cancelling 3's	Thorndike '16 Cancelling 2's	Thorndike '16 Cancelling 3's	Thorndike '16 Addition	Thorndike '16 Multiplication	Thorndike '10 Addition	Whitley '11 Maze	Whitley '11 Multiplication	Whitley '11 Cancelling A's	Thorndike '16 Typewriting	Woodrow '17 Form Sorting F. M.	Woodrow '17 Form Sorting, Normal	McCall '16 Cancelling 8's	McCall '16 Cancelling 3's	McCall '10 Addition	McCall '16 Copying Addresses	Kinsaid Braille Writing	Kinsaid Dart Throwing	Totals	
																										D	I
38	S.D.	I	I	I	D	I	I	I	I	D	I	I	I	I	I	I	D	I	I	D	D	I	I	I	I	5	19
39	$\frac{\text{S.D.}}{\text{Av.}}$	I	D	D	D	D	I	I	D	D	D	I	I	D	D	I	D	I	D	D	D	I	D	D	D	16	8
40	Ratio $\frac{\text{worst}}{\text{best}}$	I	D	D	D	D	I	I	D	D	D	I	I	D	D	D	D	I	I	I	I	I	D	D	D	14	10
41	Ratio $\frac{\text{next worst}}{\text{next best}}$	I	D	I	D	D	I	I	D	D	D	I	D	I	I	I	D	I	D	D	D	I	D	D	D	14	10
42	Gross gain	I	I	I	D	D	D	I	D	D	I	I	I	D	I	I	D	D	D	D	D	I	D	I	D	13	11
43	Per cent gain	D	D	D	D	D	D	I	D	D	D	D	D	D	D	D	D	D	D	D	D	D	D	D	D	23	1
44	Correlation between initial ability and gross gain	I	I	I	D	D	I	I	D	D	I	I	I	D	I	I	D	D	D	D	D	I	D	I	D	12	12
45	Correlation between initial ability and per cent gain	D	D	D	D	D	D	I	D	D	D	D	D	D	D	D	D	D	D	D	D	I	D	D	D	22	2
																										119	73

Rows 5–10 contain the coefficients of correlation [1] necessary for the expression of the relation between initial and final ability. Rows 7 and 10 show true correlations which are, with one exception, uniformly positive, and in the majority of cases, high. This means, of course, that the relative rankings of the members of the group are affected only slightly in some cases, and others practically not at all by the addition of presumably equal amounts of practice. As we should expect, there seems to be a slight tendency of the experiments with the greater number of practice periods to show lower correlations between initial and final ability. It is, of course, impossible to compare the amount of practice in two dissimilar learning experiments directly, since, *e.g.* 10 minutes' practice in cancelling *A*'s is certainly not equal to 10 minutes' practice in playing the piano.

The general question of the relation of final to initial ability is of practical as well as of theoretical importance. It receives attention for instance from industrialists from the point of view of adequate selection of individuals for training along given lines. The question is, of course, how closely it is possible to predict future achievement by means of an initial test. The answer which receives support from the result of this investigation is that if the scores in the initial practice period have a high coefficient of reliability as may be shown, *e.g.* by correlation with the scores of the next practice period, then there is a strong tendency for individuals who are relatively superior at the beginning to hold this superiority at the end. Correlation between initial and final ability tells us nothing exact regarding the absolute

[1] Correlations for all studies with the exception of the last three have been computed by the rank method.

$$\text{Formula: } \rho = 1 - \frac{6\Sigma d^2}{n(n^2 - 1)}.$$

Correlations for the McCall studies, Braille writing, and dart throwing have been computed by the Pearson product-moment method.

$$\text{Formula: } r = \frac{\frac{\Sigma x'y'}{N} - c_x c_y}{\sigma_x \sigma_y}.$$

In all correlations, chance errors have been compensated for by correcting for attenuation.

changes between members of the group. The question of the exact degree of improvability is a different question.

In Section *B* of Table II we have expressed the relations between initial and final differences in seven different ways: (1) comparison of standard deviations; (2) comparison of relative variabilities (= S.D./Av.); (3) comparison of initial ratio between worse and better scores and final ratio between worse and better scores; (4) comparison of gross gains made by the highest and the lowest 25 per cent of the group; (5) comparison of percentage gains made by the highest and the lowest 25 per cent of the group; (6) correlation between initial ability and gross gain; (7) correlation between initial ability and percentage gain.

Facts indicating a general prevailing tendency to increase of differences with practice are found under only one form of comparison; a larger standard deviation is found at the end of the practice period than at the beginning in 19 out of 24 investigations.

Facts indicating a general prevailing tendency to decrease of differences are found as follows: (1) There is a larger coefficient of relative variability at the beginning of the practice series in 16 out of 24 investigations. (2) The range of variability as expressed by ratios of worse to better records shows an increase with practice in 28 out of 48 comparisons. Of these the ratio of the worst to the best record is greater at the end of practice in 14 out of 24 investigations, while the final ratio of the next worst to the next best is also greater than the initial in 14 out of 24 investigations. (3) Greater gross gains are shown by the initially lowest 25 per cent as compared with the initially highest 25 per cent of the group in 13 out of 24 investigations, while greater percentage gains, *i.e.* ratios of gross gains to initial ability, are shown by the initially lowest group in 23 out of 24 cases. (4) Negative correlations between initial ability and percentage gain occur in 22 cases, while positive correlations are found twice. The average of the 22 negative correlations is $-.49$; the average of the two positive correlations is $+.21$.

Evidence obtained from the correlation of initial ability

with gross gains is inconclusive, since in twelve investigations there are positive correlations, indicating increase of differences, while in the other twelve there are negative correlations, indicating a decrease of differences. Moreover, the P.E.'s of these correlations are so high as to render the reliability almost negligible. With an average positive correlation of + .26, there is an average P.E. of .13; with an average negative correlation of − .31, an average P.E. of .11.

The weight of the summarized evidence would seem to fall in favor of the decrease of differences between individuals as being the more common result of the addition of equal amounts of practice.

As is shown in Table II, Section *C*, in 119 cases a decrease of differences is found, in 73 an increase. Of these measures, some involve absolute gains (S.D., ratios, gross gains, and correlation of initial ability with gross gains), whereas others (S.D./Av., percentage gains, and correlation of initial ability with percentage gains) measure improvement in terms proportional to initial ability. In the case of the absolute measures, the indications are that differences increase with practice about as often as they decrease; in 62 cases an increase is shown, in 58 a decrease. If relative measures are used, individuals who are initially low in achievement generally gain more with reference to their initial ability than do individuals who are superior at the start; in 61 cases a decrease in differences is shown as against 11 cases where an increase is indicated.

With further analysis of the facts bearing upon our main problem, the seven measures which we have employed, are seen to fall into four main groups.

I. Comparison of absolute differences at the beginning and end of practice
   a. Comparison of S.D.'s
II. Comparison of relative differences at the beginning and end of practice
   a. Comparison of relative variabilities—S.D./Av.
   b. Comparison of initial ratio between worse and better scores and final ratio between worse and better scores

III. Relation of initial ability and gross gain
  a. Comparison of gross gains made by the highest and the lowest 25 per cent of the group
  b. Correlation between initial ability and gross gain

IV. Relation between initial ability and relative gain
  a. Comparison of percentage gains made by the highest and the lowest 25 per cent of the group
  b. Correlation between initial ability and percentage gain

Of the four groups, I is the only one in which a strong tendency is shown for differences to increase with practice. But we are particularly seeking an answer to the question: Which is of more importance in determining the individual differences in ability found at the beginning of an experiment, hereditary capacity or amount and kind of practice previous to the experiment?

If we should find an increase of differences, as measured by the comparisons in groups I and II, we should have an indication that differences in native capacities for learning are important, but we should not have clear proof that the differences in ability before experimental practice were mainly due to native differences.

If, however, we should find an increase of differences in reliable comparisons made in group III, and even more so in group IV, we should have a strong indication that original capacities were more strongly operative in determining differences in attainments in the functions tested than were previous inequalities of practice. Of the four measures included in these groups, the two in group IV show a tendency toward a decrease of differences in a large majority of investigations, and in many of these a very decided tendency.

Group III is then a critical group for the supporters of Thorndike's view—that practice renders differences greater rather than smaller. In III, *a*, the cases are nearly evenly divided, a slight majority being on the side of decreasing differences. In III, *b*, exactly half of the investigations are ranged on each side. We have to determine now which of two alternatives is indicated. (1) There may be a general tendency for gross gains to be of practically the same size

in the two groups, perhaps a little larger for the initially poorer, *i.e.* there may be no significant correlation, or else a very small negative correlation between initial ability and gross gain. (2) There may be an unmistakeable tendency to greater gains for the better subjects under some conditions, and under other conditions an opposite tendency.

Our acceptance of one or the other of these alternatives will rest largely on an examination of the reliability of the results of the separate investigations. Since the correlation method of comparison is a more refined method, taking into account all of the individual cases, and since there is a well-established method for computing P.E.'s with this form of comparison, the decision seems to depend primarily upon the measures of III, *b*. An examination of the P.E.'s accompanying the correlations in this group of measures leads us to the observation that in the whole set of 24 investigations there is only one in which there is a positive correlation so much larger than its P.E. as to be considered reliable, or by itself significant, namely, the correlation found in McCall's experiment on addition ($r = .57 \pm .05$). The other investigations, including none that are reliable singly for the side of Thorndike's contention, must be combined and interpreted as a group. They indicate a slight tendency for the initially poorer subjects to make greater gross gains than do the initially better. In favor of Thorndike's position there stands the one clear case of McCall's experiment in addition. The question as to what gave the experiment its distinctive character would seem to be with the present evidence a hopeless one.

The general result of our analysis must be as follows. In the investigations of practice in various functions which we have attempted to study with a view to determining whether original nature or some environmental factor is chiefly operative in causing the differences in achievement which we find among individuals, it has been found that in the main there is a tendency for differences to decrease as the result of a period of equal amounts of training for the group.

In a minority of the experiments there has been fair

evidence of a slight tendency for the superior individuals to increase their lead with the addition of equal units of practice. In just one experiment the evidence has pointed strongly to such a tendency. So far as this evidence goes, it indicates most clearly that differences in original nature—in inherited capacities and abilities to learn—have been more important in producing the differences which are found at the beginning of the practice periods than has training in the specific functions.

The negative findings in the other experiments leave it an open question as to whether the above conclusion may be drawn in these cases also. As was stated at the beginning of this paper, the finding of a general tendency for differences to decrease under the particular form of treatment here applied, leaves the issue as to the cause of these differences to be determined by other kinds of evidence. So much the more is the question unsolved as to the cause in general of differences in individual attainment in socially important kinds of skill.[1]

Summary

Experimental investigations have yielded varying answers to the question: Do individual differences increase or decrease as the result of the addition of equal amounts of practice? An explanation for this disagreement is apparently not to be found in the experimental data here presented among such factors as kind of subject, type of function investigated, length of practice, *etc.* The question may be raised whether it may be found in certain experimental deficiencies, such as the use of too few subjects, too few practice periods, *etc.*, or whether it may be due to statistical misinterpretations.

It is again proved in this paper, as previously by Whitley, that no single measure adequately represents the changes in differences between individuals from one point to another in the learning process. It is always desirable, therefore, that

[1] It may be noted by comparison of Table I with Table II, Section *C*, that in a few cases a recombination of the data in order to obtain reliable measures of initial and final ability has altered the direction of the result. This indicates the necessity of basing conclusions only upon data of demonstrated reliability.

XUM

the relation between initial ability and improvement should be expressed in two or more different ways.

Selection of learning data to serve as bases for statistical investigations is seriously limited by the failure of investigators to make available full tables of original scores. The scope of the present investigation is circumscribed by reason of this deficiency.

As the result of a statistical study of 24 sets of learning data, in which the measures of initial and final ability have a high reliability, we conclude that there exists in general a high degree of correlation between ability at the beginning and at the end of relatively brief periods of practice. Predictability of later relative positions from reliable initial scores is comparatively safe.

By applying seven measures of the changes in differences between members of a group to these same data, we have shown that there is on the whole a preponderance of evidence among these cases for the conclusion that differences generally decrease with practice. This result leaves the question of the relative importance of heredity and environment in determining these differences to be settled by other evidence.

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XUM

A PHYSIOLOGICAL–BEHAVIORISTIC DESCRIPTION OF THINKING

BY J. F. DASHIELL

University of North Carolina

Latter-day reinterpretations of the process of thinking have been suggested along two different lines. The various classical descriptions of thought from the Aristotelian down to that of the contemporary analytical psychology have shown in varying degrees the interpretation of it as intellectualistic and as self-contained. Of late years the writings of the pragmatists (whose philosophy really 'takes off' from this psychological insight) have made clearer in its implications for a *Weltanschauung* the viewpoint of the functional school in psychology. For this school "reasoning is always for a subjective interest," "thinking is set up in situations of stress and problems," "ideas, meanings are cues or leads to further dealing with the object or situation," "inductive reasoning is the seeking for a general interpretation under which the given data may assume some shape that can be dealt with effectively," etc. A very different line of reinterpretation is that taken by the behaviorists. For them "thinking is a matter of implicit responses," "one's reactions to a stimulus may be shunted away from the grosser skeletal musculature and through the more refined musculature of the vocal apparatus," "habits of thought are laryngeal habits," etc.

The behavioristic emphasis—or, more accurately, the emphasis of the behaviorists—has been upon a certain type of physiological process as the essential thing in thinking. Now as has been recognized by Tolman and others, a behaviorism is not necessarily nor essentially physiological; its legitimate primary emphasis is upon the importance of the 'biological situation' in which the organism 'behaves' with reference to its environmental circumstances. (Here the line of emphasis

would seem to merge with that of the functionalists.) Whatever intraorganic processes are found to occur may be of great value in bridging gaps in the purely behavioristic account of phenomena, but as intraorganic they are not behavior. A confusion of the converse sort seems also to be abroad. Emphasis upon a physiological description as being in the last analysis the only explanatory description is widely enough held among all brands of psychologists; and any particular line of emphasis on the physiological side, such as upon the part played by vocal mechanisms, does not legitimately set one off as a behaviorist.

In expansion of this point attention should be called to another current misunderstanding. The ascription of thinking to vocal, gestural, or other bodily processes is not in itself any challenge to the legitimacy of introspective descriptions for the plain reason that this is only a move to substitute what is thought to be a more adequate physiological basis for a less adequate one. Stripped of other motives that are not necessarily and essentially affiliated with this, we have here only a revision of one of the assumptions of practically all psychological doctrines heretofore, namely, that the physiological mechanism underlying thought processes is the cerebrum and the cerebrum alone; and a recasting of this into the form of a more inclusive and extensive mechanism. *The change is from one physiological to another physiological description; and no further implications necessarily follow as to the value or as to the legitimacy of descriptions of thinking in terms of the conscious experiences of the thinker.* Confusion on this really obvious point is, the writer feels, at the bottom of some contemporary discussion.

Let us look more closely at this change in the physiological point of view. On the traditional conception, the bodily mechanism at work when one thinks consists of one or more neural impulses that reach some area of the cortex, then, instead of passing more or less immediately to some motor organs as in activity involving unthinking awareness, shift along association fibres to another cortical area, thence to another, etc.; making a picture of impulses shooting back

XUM

and forth, here, there, and elsewhere, all between innumerable cortical centers, and the whole field of operation being comprised within the cerebrum. With the realization of the ludicrousness of reasoning conceived in the ancient classical way as a self-contained and self-maintained operation, recognition has been increasingly allowed to the functions of some sensory stimulus *to* the reasoning and some motor outcome *from* it; but during the process of reasoning itself, *i.e.* after the afferent nerves had delivered their burden to the cerebral centers and before the efferent nerves had received theirs for conveyance to effectors, these peripheral mechanisms have still been considered inoperative and inactive (so far as the above thinking process is concerned). The succession of ideas experienced by the subject in his chain of reasoning is then traceable to (or paralleled by, or the other aspect of) the succession of cells or of cell-constellations at which the shifting, shunting nervous impulses arrive and leave. It should legitimately follow that the anatomical locus of a given idea (so far as it has any) is some theoretically localizable cluster of cortical neurons; but protests against this inference may be respected without affecting the point at issue.

An incidental query concerning this mode of interpretation may be stated. What factor or factors determine that the impulses shall at certain moments in this story continue to shoot about inside the cerebrum and, on the other hand, at a certain moment leave the cerebrum by efferent channels? Is it wholly a matter of what cells happen to be traversed, so that when the neural impulses chance to travel to motor centers they are then transferred to the peripherally bound tracts, but when on the contrary they travel to non-motor centers the nature of the connections made there are such as to re-direct them to other parts of the cerebrum? Or, shall we look for some inhibitory factor—perhaps in the form of certain other nerve impulses a-going inside the cerebrum inhibiting the first? But this problem is incidental.

As a contrast to this *intracerebral* view of the physiological aspect of thinking, let us consider the *peripheral* view. It

might be more accurately styled a peripheral-central-peripheral-central-peripheral view. In essence, the difference between the intracerebral interpretation and this one can be put in terms of the stimulus-response circuit concept. For the former, thinking is statable as a complication, refinement, prolongation, elaboration, etc., of the *central* segment of the whole 'reflex arc.' The simpler human activities involve fairly close and immediate connections between afferent and efferent pathways, shading down to the simplest reflexes; but as organization or integration of these simpler acts into more complex ones progresses, the central associative phases become more and more important, until in deliberative thinking these central connections are found to be indefinitely complicated, involving in part a great elongation of the total pathway traversed by the impulse from the place and time of its first arrival at the center to the place and time of its ultimate leaving the center. In contrast with this, on the peripheral interpretation the associative pathways serve merely as connectors between peripheral tracts; the connectors being subject to an enormous amount of modification, to be sure, but this modification being largely limited in character to the joining and disjoining of simultaneously and successively operating central connections, and little if any to the hitching end-to-end of merely central paths in a serial order, as according to the other view. In other words, the emphasis is here upon *whole arcs*. Modification of human activity is on the anatomical side largely describable as joinings and disjoinings of whole arcs. Where modification does produce a serial pattern of organization, this is due to a serial hitching together of the whole arcs.

This description of the physical side of the process of thinking in terms of the interplay of entire sensori-motor circuits instead of merely the central segments thereof, possesses on its face the advantage of fitting in with the undeniable modern tendency to interpret psychological phenomena as reducible to a matter of stimulus-and-response circuits.

The train of ideas and the mental seesaw of deliberation

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experienced by the thinking person, are on this physical view, a matter of sensori-central-motor arcs active in various relations to each other. Let us consider two traits of thinking that have been universally remarked.

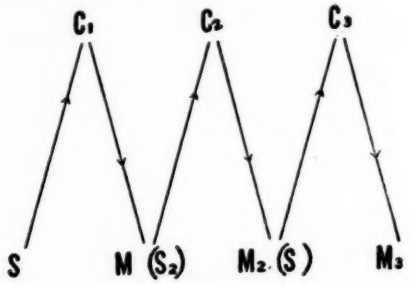

FIG. 1

(*A*) In the first place, there is the phenomenon of 'thoughts' in series, a train of 'ideas.' This is theoretically describable on the physiological side as a series of sensori-motor activities, occurring in succession. In pattern the 'ideas' resemble (in last analysis, are) the part-processes in a serial habit. A diagram of the latter will help to clarify the point (see Fig. 1). An afferent impulse originating at peripheral sense-organ *S*, arouses by central connection *C*, the motor response at *M*; this indirectly—through excitation of receptors resident in a muscle tissue by the very contraction of the latter, or through excitation of receptors situated in the vicinity and sensitive to certain changes produced in the vicinity by the effectors—arouses a second sensory impulse from *S*, which in turn arouses through a center *C*, another motor response *M*, etc. Now, according to this view, the serial character of thinking is physiologically a process to which this scheme is applicable, the sequence of experiences the subject calls his 'ideas' being describable as the sequence of motor adjustments arousing each other in turn, and not as the sequence of intracerebral shuntings of associative impulses represented in Fig. 2. A challenge may be anticipated: How can an idea be represented physiologically as the activity of an arc? To such a challenge the most

convenient retort would be: How, on the other hand, can an idea be represented physiologically as the activity of a group of cortical neurons or as the passage of an association impulse? A more serious attempt to meet the need will be made below.

Meanwhile we must consider the other well-recognized trait of thinking taken in its narrower sense: (*B*) that thinking involves some seesaw, some competing alternatives. Sher-

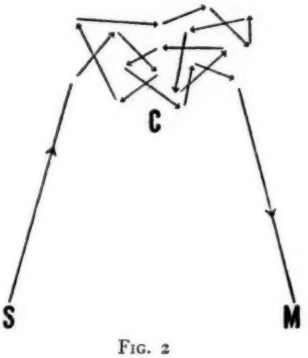

FIG. 2

rington has familiarized us with the notion of reaction systems as being allied or antagonistic, reinforcing and inhibiting with reference to each other. And nothing resembles the introspective phenomena of thinking more than some of the interrelations of reaction arcs. On the one hand, the subject reports hesitations, the pondering of alternative suggestions, and the final acceptance of one either in the non-logical decision or logically after a new consideration has appeared and strengthened that alternative; on the other hand, we can observe antagonistic reaction systems blocking each other on the reception of certain stimuli, other systems meanwhile constantly varying in their tension until some one of these waxes strong enough to reinforce one of the original antagonistic units so that it may appropriate the 'common path.'

But observe! The 'consideration' that enters to reinforce successfully one of the competing tendencies is in the more

logical types of judgment not wholly from 'without' the individual, not merely an enhancement of an external environmental stimulation, but is from 'within' his own 'mind.' Clearly, then, we must make room for just those 'inside' reaction systems pointed out in the diagrams above.

The James-Lange theory of the emotions, especially the broader statement of the position by James, has in it much more suggestiveness than the discussions thereof have ever evidenced—suggestiveness for the interpretation of far more psychology than is connoted by the term 'emotional.' Indeed, we are tempted to assert that that 'theory' (still so-called, although it should surely deserve by this day to be called a law!) could without much alteration be used as the central core of all psychology. The point may be summarized in advance: the representation of the phenomena of psychology in terms of (external) stimulus and (overt) response that has had such vogue is grossly inadequate; it *fails to take account of intraorganic reactions and stimulations, and of the fact that most objectively observable stimulus-response phenomena have interlarded layers of shunt line processes via the viscera and soma.*

But to make the point clearer let it be stated in more detail. First, let us make a statement of the James doctrine of emotion. It consists essentially of the two propositions that external stimuli can and do arouse a variety of 'inside' changes, and that alterations of 'inside' physiological activity can be and are in many cases the sources of afferent impulses leading back to the central system. Stated in some such way this doctrine avoids the objections of the extreme behaviorists who can see nothing valuable in it; it does not stand or fall with the legitimacy of the introspective approach, for the real contribution consists of James' insight into this one fundamental thing—that an afferent impulse from an exteroceptor does not discharge completely into paths to effectors producing overt reactions, but in some degree discharges into efferent paths leading to motor organs internally placed and produces there changes that usually arouse adjacent sense-organs, these in turn bombarding the central

system and contributing this internal quota to the sum total of mutually reinforcing and inhibiting processes a-going through the centers. "A process set up anywhere in the centers reverberates everywhere, and in some way or other affects the organism throughout, making its activities either greater or less." That the subject is *aware* of these internally aroused afferent impulses, as implied in many of James' statements of the doctrine, is after all not essential to this insight and should not damn it for those who are sensitive of any references to the experiential side.

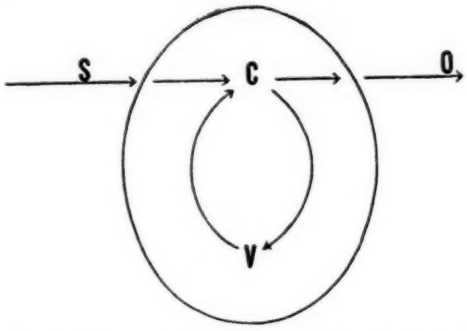

FIG. 3. Stage I. The afferent impulse aroused by the stimulus *S* may at *C* lead immediately to overt action upon object *O*, but also may lead to reaction of viscera *V*, in turn furnishing new afferent supply to and through *C*.

Stage II. This new afferent supply inevitably affects and is affected by other impulses in passage, thus modifying the overt reaction on *O* as well as further modifying the internal changes.

To make clearer the fact that this conception of emotion may without loss of essence be stated in wholly objective terms, the writer has schematized the matter down to its skeleton in Figure 3, and has introduced some details in Figure 4.

Before applying this analysis of emotion to the description of thinking we must get the process of thinking 'placed' in a general way, its locus in the general life economy of the human being. The emphasis placed by the functional psychology upon the part thinking plays in the life history of the individual would seem to be as genuinely 'behavioristic'

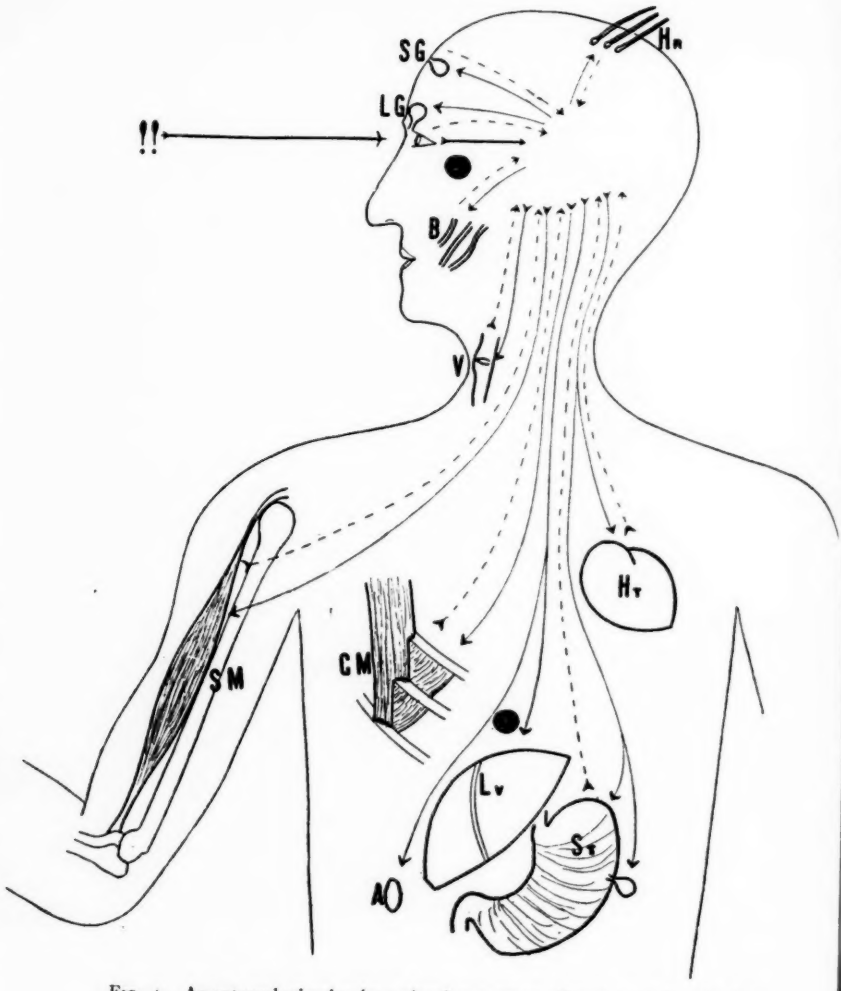

FIG. 4. An external stimulus (as a ghost) arouses an afferent nerve impulse from eye to brain in turn arousing a team of motor pathways. These then produce changes in lachrymal gland (*LG*), in sweat glands (*SG*), in position of hair on head (*Hr*), in blood vessels of skin as in blanching (*B*), in skeletal muscles as in straining, 'excited' activity (*SM*), in heart beat (*Ht*), in stomach's digestion involving both muscles and glands (*St*), in secretion of adrenin at (*A*) and glycogen from liver (*Lv*) into blood, in skeletal muscles of chest controlling breathing (*CM*), in vocal sounds from larynx (*V*), etc.

as the newer physiological conception stated in the first pages of this paper. At any rate, the two treatments of the subject dovetail excellently, and in the writer's opinion, strengthen each other, the one stating the original occasion as well as the final outcome of thinking, the other describing the particular processes so occasioned and leading to such an outcome.

The type example of the functionalist's interpretation of thinking is that of a man baffled by some characteristic of the environment. Characteristically, if his behavior is of a rational type, he is not content to kick and squeal, pull and tug, run to and fro; he 'stops to think it over,' he may 'sit down to consider.' Does this mean that all motor activity is stopped? Hardly! It means rather that it is 'toned down,' partially inhibited from full explosion. The degree to which this abbreviation of his acts is carried varies, of course, by all gradations. At one time we may observe him tentatively but actually starting to do this, then that, then the other; again, we may see him sitting still but shifting his eyes here, there, elsewhere, with slight turnings of head and body, with minor movements of fingers and hand; or, he may by all superficial appearances be motionless and his reactions escape all observation but that aided by delicate instrumentation applied to tongue, larynx, artery, chest, and finger tips. Then at the conclusion of this period of more or less implicit reactions our man may be seen to resume his more large-scale, gross behavior, and he deals with the situation now in some definitive and manifest way; his problem is solved and he is acting accordingly.

No consideration of thinking can proceed far without a wrestle with the problem of '*meaning*.' To be sure, a behaviorist (Watson) may deny the usefulness of the term and insist that "the question of meaning is an abstraction, a

Dotted lines show how the changes aroused in different parts of the body may contribute new afferent impulses to the centers. In terms of the usual analysis of the subject's awareness, these secondary impulses give him the feeling or emotional quality modifying or coloring the externally aroused sensations. Objectively speaking, these secondary impulses now play a modifying part in further stimulus-response phenomena.

XUM

rationalization and a speculation serving no useful scientific purpose. . . . From the . . . behaviorist's point of view the problem never arises. We watch what the animal or human being is doing. He means what he does. . . . His action is the meaning." Now, as shown above, frequently the action does not occur, or does not completely occur; hence not only is his action but also are his tendencies-to-action and his general motor attitude the meaning of the stimulus. At any rate, the term serves as a useful shorthand expression; and no doubt the behaviorist's discarding of it is motivated in part by the fact that it has heretofore been used almost exclusively in a structuralistic and introspective manner. Some tendency to break away from the use of the term as referring only to certain aspects of conscious experience has been shown in the seeking of description of it as an 'attitude,' an 'adjustment,' as by Bolton.

An excellent approach to this position is via the study of children's definitions. It is too well known for elaboration that the child of four or six will define nearly everything he is questioned on in terms of its use, *i.e.* in terms of his own dealings with it. A chair is 'what you sit on,' a horse is 'to ride on,' a pencil is 'something you can write with,' etc. A child of the writer's during his second December frequently telephoned Santa Claus a detailed schedule of Christmas expectations, and whenever he mentioned the word 'football' involuntarily kicked the wall. It may be maintained that that is what a football *is*—a kickable thing; should such an article become permanently deflated it has lost (in Aristotelian phraseology) its virtue, it is no longer a football but a wad of stuff to be thrown away; and the naïve youngster will turn into a 'football' anything that is kickable, whether describable by his elders as made of leather-and-rubber, of black rubber only, of a hog's bladder, or of a cotton stuffed calico bag. To be sure, the adults do apply these names of composition and structure, but are not they and all other 'ideas' or terms really *shorthand symbols for the mass of dealings-with* that the adult may at one time or another employ, a naming reaction that has by conditioning come to do duty for a mass of overt manipulations?

It is now high time to be calling attention to a new point in our description of thinking, a point that has all along been lurking in the background. No small part of a child's reaction upon a thing, of his attitude toward it on later occasions, or of the meaning it has for him when mentioned, is the affective-emotional phase. And we can extend the point to cover the topic of perceiving in general. The skull-and-crossbones in red on a poison label, the funniness of the clown, the attention-arresting character of the name LOUVAIN in the latter months of 1914, all these point us to the observation that these affective aspects of a thing perceived do not merely *accompany* the perceiving act but are part and parcel *of* it. One might put this still more vigorously by showing that one's perception of a thing is dependent upon this as one of the integral factors; the meaning given a thing is based upon the overt motor responses that thing arouses in whole or part and also upon the covert visceral and somatic responses it arouses. Going back again to our Fig. 4, it is evident that the stimulating thing really arouses *one complex* response (or series of responses), each response then being only formally analyzable for pedagogical or experimental purposes in psychology into the more observable 'outside' and the more private 'inside' parts of the whole. In human experience, the sun is first of all a cheery fellow, greeting us in the morning, warming us in the chilly breeze, mayhap sunburning us at times, ripening our corn, drying our clothes, playing generally a striking part in this world of weal and woe and of things-to-be-done. Only with later stages of mental development when most habits become abbreviated and short-circuited and symbolized under the needs of social communication and of greater rapidity of action does the sun become the incandescent body in space familiar to us in astronomy. On this evolution, some words below.

The foregoing remarks suggest that we are face to face again with one of those distinctions of convenience which have so often in the history of knowledge become crystallized and fossilized into hard and fast divisions. This attack upon the

over-intellectualized psychology of perceiving carries within its womb—and for the present the writer will not play the part of a mid-wife—a discussion matter involving fundamentals of all brands of psychology, at least all approached by introspective methods. Perhaps, after all, there is no legitimate and final line of demarcation between 'cognitive' and 'affective,' between intellect and emotion, any more than there is a clear line between the motor responses called overt and those called implicit. Has the distinction not been drawn originally as one of convenience?

Viewed objectively, *the 'meaning' of a thing is* determined by—is constituted of—*the whole motor-emotive set or attitude aroused by that thing*, these attitudes being nothing other than habits formed in response to the thing.

Language.—One of the commonest recognitions in the history of psychology has been that of the peculiar intimacy of thought development and language development. And such questions have at times been posed as, Can one have thoughts without words to frame them by? Yet the possibility of reducing them one to the other, making them not one the real process and the other only its tool, but both merely two ways of regarding the same human performance, had not vividly possessed psychologists—due, no doubt, to that lurking feeling of the sacrosanct and always-somewhat-mysterious about thinking. Today, however, it is becoming increasingly evident to some that in language itself, in its origins and developments, we may find the key to the problem of the physiological processes we call thinking.

To understand the original and primitive significance of language we must first be reminded of those inherited pattern-reactions that may be styled instinctive-emotional (again, with these two words we have with us a petrified distinction that in truth cannot be made out in definite lines). Now, given animals or humans manifesting these earliest appearing tendencies in behavior, tendencies to reactions in manifold ways differing by all degrees in observableness from the gross movements of pushing away by arm or leg to the subtle changing of a hormone secretion; given also a situation in

which these organisms live in proximity so that they may stimulate and react upon each other; and an exceedingly important series of phenomena occurs. Let us imagine that Rover is eating a piece of meat, and assume that none of the learning process we want to trace has yet taken place on his part nor on that of the dog, Towser, appearing in the offing. What is more inevitable than that Towser, whose olfactory and visual receptors are assailed by the food, should approach and bite into the meat? But now the situation is accurately set for arousing in Rover (and, it may be assumed, not as an acquired but as a native tendency) a pugnacious or attacking response (involving the whole complex of visceral-and-skeletal action systems) and it proceeds to the point where an effectual biting attack has removed the Towser obstacle or interruption. Here occurs a change in the latter's neuromotor organization known by the term 'conditioning'; the original situation for Towser in which his cutaneous sense-organs were receiving the intense pain stimulation and thus arousing his own withdrawal movements included also such incidental stimuli as the visual from bristling hairs and bared teeth on Rover's part and the auditory from the latter's growl; and on later occasions of similar character the representation merely of growl and bristling hair may be potent to excite the complete act of withdrawal.

Henceforth, whenever Towser's proximity is objectionable Rover need only growl and bristle to control the former's conduct satisfactorily, and the growling-and-bristling soon becomes an habitual attitude, usable toward a variety of animals and persons and usable in situations not actually demanding a full biting attack, but in which a retreating behavior by the other subject is desirable. And now, if only Rover were able to want intentionally to frighten the 'other-fellow,' he could deliberately do it by voluntary control of his growling and his bristling mechanisms. At all events, this intentional using of *some* of the whole complex of overt-and-visceral reactions involved in actual direct dealing with the other-one is observable early enough in childhood; crying to bring mother, drawing back hand to threaten a younger

brother, forcing a smile to secure more favorable treatment—such instances are legion.

If there is this development of ability to use *abbreviations of instinctive-emotional reactions to influence others* there is to be remarked also the development of ability to be sensitive to these abbreviated acts when made on the part of others. Indeed, so essential in human social life is the development of both the active and the receptive sides that we find children surprisingly quick to read and to convey meanings with gestures and vocal sounds, much quicker than to do so with articulate words. We may conceive the latter as artificial symbols, shorthand indications of the former; and it is interesting to observe how in different social cultures varying amounts of this substitution have occurred, from the American Indian of many grunts, shrugs, manual gestures and few words, to the Chinese or Englishmen of stolid face, reserving his meanings, his personal attitudes to be detected almost only from his articulate vocal sounds.

This interstimulation and response (suggested years ago by Mead) becomes a very broad fact, inclusive of a wide range of phenomena of social nature. A few illustrations may assist us. In a flirtation both individuals put their best foot forward and with sufficient tentativeness; A's advances being made first with hardly noticeable casualness, then, if B's reactions thereto be not of an unfavorable tenor, later with more boldness, but always the bits of conduct of each principal serving in turn as cues to the other. Consider the attitudes of not-too-pugnacious men found facing each other in a quarrel that has collected a crowd: let one lower more darkly and approach ever so little, the other visibly grows more tense and pugnacious, then let either relax from this fist-clenched vigilance and the other is fairly sure to follow with the same degree of relaxation. Incidentally, we find in this general description of social interstimulation and response the principle of 'minimal stimuli.' In ballroom dancing, the leader may be largely unaware of his slight changes of movement of right or left hand or of body generally, yet these serve as sufficient cues to his partner so that without being

aware of them on her own part she nevertheless follows accurately.

The development of articulate language proceeds largely out of this social setting. By degrees certain vocal signs come to be the symbols for certain motor-emotive attitudes, and thus gesture and the more native vocal sounds tend to be replaced by these conventionalized signals. We need not here follow up this development, but in general we see the stabilizing and conventionalizing of human social attitudes, the evolving of artificial signs to signify personal motor-emotive reaction tendencies.

For an understanding of the process of thinking, now, it is essential to note one aspect of the further development of language: the *gradual shift from overt speech and gesture to covert forms*. The movements and gross bodily acts that become conveniently abbreviated and delimited to vocal and manual signs to the other fellow, in certain social situations gradually become further condensed and more nascent. The average three-year-old has progressed far in the substituting of mimetic movements and verbal reactions for his earlier actual manipulations of objects; but he is due to go much further in this direction of abridgment, through the mumbling, *sotto voce*, and the lip-moving stages to that of silent reading and silent speech.

It is important to note that this abbreviating of behavior now is no longer principally a social phenomenon. It is observable characteristically when the child is playing by himself. While building his blocks or adjusting his doll clothes, his activity is interleaved with words, with facial grimaces, with manual demonstrations; some of this being of the nature merely of accompanying activity, but much of it too of an anticipatory and a tentative sort. By degrees it becomes evident that *one anticipatory act may lead to another, this to a third*, and so on; and the same interplay of stimulus and response by mere signals or symbols used in a social relationship, now appears *within the one child's own organism*. The sight of a block askew may initiate an emotionally facilitated act of knocking the whole thing over, but this may

XUM

not get beyond the mere start to do it, on account of a 'set' to keep on building based on the previous building acts; the left hand may start to adjust the poorly placed block, only to be 'headed off' before fairly begun by the influence of a developing dextrality shown in the using of the right hand instead; in case the result of the right hand's work is to disarrange the block further we may observe a drooping mouth, a sudden expiration, a falling cadence of vocal sound, and can guess at the unobservable 'inside' changes; and some further manipulations are then called out, ending perhaps in a drawing back of body and head, a deeper breathing, a reduction of muscular tonus, and audible vocal sounds. Now, most of these acts are neither actual dealings with the situation nor signals to a social object—they are mutually interacting and reciprocally influencing within the one child alone.

When one first learns to add a column of figures he 'says' the numbers and sums aloud, but with practice subdues this pronunciation to a whispering and lip movement stage which some of us never outgrow, and later still represses even these outwardly observable vocal acts, leaving only the necessary eye excursions and pencil pointings and writings to be seen.

Evidences bearing directly toward our thesis may be found on many sides. The case of Inaudi, the lightning calculator, is instructive: Meumann reports that he was unable to do his prodigious feats in 'mental' multiplication when he was hoarse. The well-known line of experiments with the automatograph by Jastrow and others have been introduced in connection with a variety of psychological topics but they surely bear witness to the fact that the direction and play of our 'ideas' is with sufficiently adequate technique shown to be a motor phenomenon. The recognition that attention is a matter of a-tension is in point. The training of animals to respond to minimal cues, as with the Elberfeld horses or the German police dogs reported by Most, has significance for human psychology in the fact that the men present were thinking more or less overtly! Thus, too, with the aphorism that mind reading is muscle reading.

In attempting the solution of Morgan's railroad puzzle [1] students of the writer showed great individual variations, but all found themselves, or were seen to be, making motor responses throughout—in the form either of eye-ball to-and-fro movements, or of hand and finger partial pointings, or of scarcely voiced verbal tryings-out.

What, now, are the mutual interactions of these more abridged and implicit responses? They are in no wise different from the interactions of responses generally. Upon analysis the following principles are among those that appear.

1. One act or motor set may lead naturally (*i.e.* by virtue of well-formed central connections) to another—the explanation being mainly, habit. The phenomena of memory for serial impressions is an obvious example; one may add, the train of one's thoughts show some factors of association, which are none other than factors well known in determining the rearousal of old habits. The learning of a bit of 'knowledge,' of a fact, likewise requires no other principles than the conditioning of reactions.

2. One act or motor set may lead away from another. Again, witness the identity of principles involved in memory and association, on the one hand, and in the succession of acts, on the other.

3. One act or motor set may inhibit, interfere with,

[1] A one-track railroad has a switch which will hold 25 cars and an engine. This switch is connected at each end with the main track. Two 50-car trains (*A* and *B*), each having but one engine, meet and must pass at this switch. Mark in order from 1 to 9 the steps that would have to be taken to pass the trains.

.....Engine *B* couples with second half of train *A*.
.....Engine *B* uncouples from the second half of train *A* and backs out of the switch.
.....Train *A* divides into two halves.
.....Engine *B* backs train *B* over the switch until it has placed the second half of train *A* on the switch.
.....Engine *A* pulls the first section of train *A* off the switch on to the main track.
.....Engine *A* backs the first section of train *A* and couples with the second section of train *A*.
.....Engine *A* pulls the first section of train *A* on to the switch.
.....Train *B* running on the main track passes the engine and first section of train *A*.
.....Engine *B* pulls train *B* past the second section of train *A*.

This puzzle is taken from 'Morgan's Mental Test,' by John J. B. Morgan, State University of Iowa. The object of the puzzle is to rearrange the steps in correct order.

XUM

or antagonize another. The antipathetic or contradictory relations between many of the meanings we think in terms of, is paralleled physiologically by antagonistic relations between reaction systems, even those as simple as Sherrington's reflexes, and including visceral changes.

4. One act or motor set may facilitate, support, reinforce another. The same remarks apply here as in 3.

5. The integration of one's knowledge into a more and more inclusive conceptual organization is physiologically paralleled by the integration of part-reactions into higher-level habits, the hierarchical type of organization being striking in both cases.

6. The schematizing and condensing of meanings that goes on in the maturing and elaborating of one's thought life, introspectively observed, is not in essence different from the short-circuiting of part-reactions in the organization of habit-wholes. In adding, we have a borderline example of both: after adding digit to digit, one comes to 'read' his number combinations, taking in two, three, and more figures as one meaning. Similarly with the building up of word reactions in Book's pioneer study of typewriting.

Truly, it only remains for some one to work out a complete set of experimental demonstrations of the phenomena and principles of *habit making and breaking*, to show thereby that *the laws of conscious psychic life are after all nothing other than these*. Meyer has opened the way in his 'Psychology Demonstrations.'

Resumé.—1. Thinking is coming to be recognized today as a human function that involves some substituting of implicit for explicit reactions.

2. This substituting occurs most clearly when the explicit activities are inadequate, often in conflict.

3. The implicit reactions can be given a physiological treatment most consistent with other physiological facts when described in terms of reflex arcs: thinking being a matter of interacting and mutually influencing motor sets or responses, not of intracerebral shiftings of association impulses.

4. The James-Lange theory of emotion by extension of application reminds us that thinking is done by the whole body, and not by the vocal apparatus and gestural apparatus alone.

5. It reminds us also that thinking is done not merely with striped muscles but also with visceral muscular and glandular effectors, and that one cannot deny the emotional and affective contributions to the whole performance.

6. That motor-emotive responses of abbreviated types can be the determinants of meanings appears from the development of gestural and vocal communications of signs.

7. The social communication-meanings become meanings for the individual's soliloquy, and this leads to a change from overt to more or less implicit.

8. The phenomena of thinking are consequently restatable as the physiological phenomena of interacting reaction circuits, or the behavioristic phenomena of interrelated motor acts and motor sets of varying degrees of explicitness.

XUM

METHOD FOR ORGANIC PROBLEMS

BY J. A. MELROSE

Madison, Wisconsin

When Archimedes says, "Give me a place to stand and I will lift the world," his problem seems heroic because stated in heroic terms. Had he, however, tried to lift himself by his own boot tops he would have faced the same dilemma. Psychology is in this dilemma. Its fulcrum rests upon its load. It is trying to lift itself in that it *employs* '*thinking*' *to explain* '*thinking*.' It wants a 'place to stand,' a *point of objectivity* from which to operate, and until such objectivity is found, deadlock in the central problem of psychology must continue.[1]

This fact of deadlock in the central problem of human psychology rests back upon the problem of correct scientific method for treating strictly organic problems. *As yet we have no such organic method.* This is the part of our scientific technique which is lacking. Before we turn, however, to the generic question of positive method for distinctively organic problems, we ought, perhaps, for the sake of clearness to distinguish sharply the *central* problem of psychology, which is deadlocked for want of a correct method of attack, from the *secondary* problems in which most of the work in psychology has been done and for which the present method, although essentially inorganic, appears to be on the whole adequate.

The central problem of psychology is the task of analyzing psychology itself and should lead to an exact philosophy of the science. It has to do with the *life needs of the individual and the technique by which adjustments to these are made.*[2]

[1] 'The Crux of the Psychological Problem,' PSYCHOL. REV., March, 1922.

[2] The psychoanalyst appears to be attacking the central problem. He accepts the integrity of the individual in the pursuit of his life needs and is in so far correctly oriented to the central problem, but an exact method of analysis is lacking. Other

Secondary problems are strictly speaking not analyses of psychology itself but psychological analyses of related problems of our own shaping. Such problems are, for example, the classification under various heads of mental structure and functions, finding patterns of behavior, computing relative speed and accuracy in learning, measuring relative degrees of native intelligence and educational achievement, finding the relative importance of school subjects for securing gains in learning, etc.

These results do not of course tell us how the individual makes his life adjustments. They do not attack that problem. The problems attacked are not the organism's problems but the psychologist's problems. As such they are often of very great dynamic importance to education, although success is conditioned somewhat upon progress in analyzing the central problem of psychology. For example, if we were to analyze 'general intelligence' into its separate elements, we could doubtless greatly extend and refine mental measurements.

But we appear to be making no strictly scientific advance upon the central problem. Granted that the *new* psychology is attacking the central problem it can lay no claim to an exact method of analysis. The *old* psychology, on the other hand, adapts its problem to customary scientific method and so doing does not appear to attack the central problem at all.[1]

psychologists do not appear to attack the central problem at all. They begin with arbitrary and partial viewpoints such as sensation, perception, association, consciousness, muscular and glandular reaction, stimulus and response, etc., and make more or less piecemeal studies from these viewpoints. These viewpoints have their advantages both in solving secondary problems and in throwing light upon the central problem. They do not, however, attack the central problem and there is no way to arrive at conclusions for the central problem by summing up piecemeal studies.

[1] The *stimulus-response* viewpoint has proved most valuable and is now generally adopted by academic psychology. Its usefulness doubtless justifies its wide acceptance. But it should be noted that it falls short as an approach to the central problem of psychology. A stimulus is not a stimulus *per se;* nor is a response always a fixed and uniform reply to a given stimulus. A stimulus is conditioned upon the *state* ('*set*') *of the organism*, while a response is first of all the *reply of an organism*,—which organism may respond differently to the same stimulus. Both stimulus and response are conditioned both quantitatively and qualitatively by the functional 'set' (equilibrium 'set') of the organism. The synthetic state ('set') of the organism is the primary fact and it must have first consideration in our orientation if our aim is *central* analysis.

What is required is a method based point by point upon the distinctive nature of the organic datum. Our present scientific method is incomplete as a scientific technique. It is essentially an *inorganic* method. It grew up in inorganic science and is an exact method for inorganic problems and problems which can be reduced to like pattern. *It is not a positive method for distinctively organic problems.*

This paper limits itself to the genetic question of the correct method for the central analysis of organic problems. Success upon this problem should remove from human psychology the false preconceptions of method which now beset it and hinder progress.

The so-called *genetic* method is an attempted accommodation to organic requirements. It aims apparently to adapt method to the distinctive nature of the organic datum. But it is both confused and inadequate. It is usually contrasted with the *quantitative* method. This is a confusion. The contrast is at the wrong point. *All science to be exact must be quantitative, whether organic or inorganic.* Moreover, the *genetic* method is not fully organismal. It is not thoroughgoing, but continues to depend uncritically upon the old elemental method of analysis. Hence its results are descriptive and at best classificatory.

Psychology as a science has developed three distinct viewpoints, the *analytic*, the *functional*, and the *behavioristic*. Of these three, it is clear that the second or *functional* viewpoint alone places the emphasis where the organic problem requires that it should be placed. Functional adjustment is what we are studying. The adjustive problem is the organism's problem and it is therefore the only one which is strictly pertinent. In the nature of the case other viewpoints —whatever their merit—are arbitrary. This is what is the matter with *analytic* and *behavioristic* psychology as we now have them. They have been more rigorous in method than functional psychology, but their respective aims are beside the main problem. They pay no special heed to function, *i.e. how adaptation is achieved*, and this is the real problem.

In focusing attention upon adaptation rather than upon

mechanistic pattern, functional psychology was upon the right path. It did not go far enough, however. Attention became fixated upon the adaptive act when it should have pushed on to the full adaptive process, *i.e.* the *process by which the adaptive act itself was acquired.* Had this been done it would have been possible to keep the emphasis squarely upon adaptation, as it should be, and still reduce the analysis to mechanistic and exact terms by finding the pattern of the process by which the respective types of adaptation were learned. Functional psychology has failed apparently, because it did not consider the whole functional process and press back to its structural analysis, which must be done before results can become definitive. The only way to get to exact results in a dynamic science is to locate the *static* (*constant*) elements in the dynamic process. That is, in a dynamic science we must press back to *structure* in order to get our work upon a quantitative basis.

In brief, then, in a central organic problem the viewpoint must be functional but the analysis structural. We must seek structure, but in definite reference to function (adaptation), or, in other words, we must seek the *structure of the functional or adaptive process.*[1] Since we can study function only as in some way objectified in behavior, our method will be *behavioristic* but not in any doctrinaire sense.

To meet these demands of the organic problem and a positive method for attacking it, we are offering a new technique of procedure. *It is intended to stand along side of*

[1] The word *function* is used in psychology in two distinct senses. It may mean either (1) process or operation in general as contrasted with structure; or (2) adaptive process—a process which yields results useful to the creature. In this paper it is used in the latter sense. The term *functional process* is sometimes used to denote the operation of a reaction already learned provided the reaction has adaptive value. This usage seems too narrow. In the widest sense a *functional process* is a process by which the adaptive reaction has been acquired or learned quite as much as it is the process of performing the action after it has been acquired. It must therefore be studied from the incipient beginnings of 'trial' until the adaptive behavior has become a fixed mode of reaction. The term is used in this sense in this paper; it is practically synonymous with *learning process.* By *structure of function* we mean not merely the pattern of the adaptive act as performed, but also the pattern of the process by which it was acquired. The structure of function embraces the constant elements in the process by which types of learning are achieved.

XUM

our present inorganic method and complete the scientific technique. We shall call the method the *organismal* method. It is a method for analyzing central organic problems. We have applied the method to the analysis of animal learning in our paper, 'The Structure of Animal Learning.' [1] Let us describe the method as exemplified in that study.

1. The primary organic fact is the *integrity, or unitary nature* of the organism—a fact ignored by the elemental method of analysis. It is the most constant of organic facts and the one which most clearly differentiates organic from inorganic science. The *organismal* method takes its primary orientation from this fact. This is its first characteristic.

2. Accepting the organism as an integrated whole, we accordingly treat distinctive elements in this integration as secondary to the whole. Each element is therefore viewed as contained within and having a genetic history within the organism. This demands the *serial approach* to separate elements which is the second distinctive mark of the method.

3. Thirdly, the method maintains throughout the *functional viewpoint.*[2] Of course arbitrary viewpoints might be chosen in analyzing learning and some of them would be fruitful in explaining secondary problems. If, however, the aim is a central analysis of an organic problem there can be but one correct way. We must *adopt the functional problem of the organism* and analyze it. An organic fact is scientifically explained only when analyzed *with reference to its functional meaning to the organism.* Any other viewpoint is in the nature of the case an abstraction in the mind of the psychologist and out of accord with the facts to be observed.

4. Finally, our quest in this organismal method is for *structure*, in order that our results may be mechanistic and exact.

In accordance with the above facts of datum and method, our analysis of psychology logically begins at the very lowest biological levels and follows the facts up the ascending series.

[1] PSYCHOL. REV., May, 1921.

[2] That is, the viewpoint which focuses on the study of adaptive reactions and how they are acquired.

XUM

This is because of two facts hinted above—(1) the integrity of the organism, and (2) the serial relation of elements within it. The integrative action of the organism effects a smooth unity in all psychological data so that complex data cannot be analyzed by a direct attack. Besides, the separate elements we seek when analyzing learning were in fact laid in a serial order in the genetic history of the organism, and hence to begin our analysis at the bottom and move upward is to follow a certain order which the problem has within itself.

In brief, then, (1) our orientation is *organismal*, (2) our approach *serial*, (3) our emphasis *functional*, and (4) our quest, the *structure of psychological function*. Accordingly, in our paper on animal learning we began our study on the lowest biological levels, and maintaining the *organismal* viewpoint, carried our observations and analysis up through sub-human animal groups.

In this manner, we isolated six *structural types* of learning which we believe cover all the learning of which animals below man are capable. These types were named descriptively, *organic fixation*, *organic spacial adaptation*, *organic choice*, *organic association*, *organic conception*, and *organic judgment*.[1] The final two types look in motor pattern convincingly like 'thinking' and may be called '*motor thinking*.'

We shall not describe these types, which is beside the point here, but will have a word upon the actual mode of analysis by which they were isolated. It may first be clearly noted, however, that these types are not mere *patterns of behavior*. They are *learning patterns*. They are not patterns of behavior, but the patterns of *gains* in behavior. That is to say, these types are patterns of the learning itself—*the constant elements in the processes by which growing skill in adaptation is achieved*.

The actual technique of analysis by this *organismal* method

[1] The names assigned to these types are suited to the description of animal learning. The same types operate also of course as part of the human learning technique. In human learning they have the same fundamental pattern, but might to advantage be described in less motor terms provided we keep in mind the genesis of the respective types in lower learning.

XUM

is simple enough and may be easily described. The analysis itself, however, is not so simple because of the subtle nature of organic unity from which we must draw the hidden structure. The method is roughly as follows.

Learning is 'modifiability of behavior upon the basis of experience.' To find the lowest learning we observe the behavior of the lowest organisms. Some of these creatures appear to have little learning worthy of the name. They inherit their simple reaction-systems in completed form. By means of reflex 'avoiding reactions' they adapt by 'trial,' but in so doing learn no new behavior. These types cannot interest us here for we are interested only in *behavior which reveals learning.* When we have found behavior which seems to represent the lowest reactions that contain learning, we observe this repeatedly until we have marked the *constant factors* in the process by which the gains are made. This we can do unobstructed at this lowest level, for the learning is simple. It represents a single type and offers nothing for us to unscramble. These constant elements give us the *structure* or *type* of this lowest learning.

Having in the above manner isolated Type One, we look in turn for the next higher behavior involving learning. This behavior will of course be more complex. It is an integration, —containing as it does Type One, already isolated, and Type Two which marks this learning as a step in advance. *This is the typical organic problem.* We must separate elements which have been fused into a smooth whole by the organism. The gross behavior appears to be a unity. All joinery is hidden. Our task is to isolate from this behavior—a unit to direct observation—the distinctive learning types which it contains.

We have already analyzed Type One and know what it is in exact terms. We have before us a gross behavior datum containing Type One and Type Two. The problem is to isolate clearly Type Two. How shall we proceed?

We first eliminate from our problem all the behavior for which the first type of learning is adequate to account. This we can do—although it may require repeated observation—for

we have already isolated this type and definitively analyzed it. This done, we, of course, have an unexplained remainder for which the new type is deemed responsible. We then observe in turn this unexplained behavior until we distinguish clearly the constant elements in the learning which it contains. These constant factors give the structure of Type Number Two.

In this manner, we proceed up the whole learning series. We need not describe the process further. Our description so far is typical. The analysis itself is by no means always easy, but on the contrary clearly grows more complex as we move up into more and more complex data. The unity remains the same smooth unity of organic integration, but it contains, as we move upward, an increasing number of elements to be isolated by this serial method.

The technique of method is simple, however, and is fully exemplified above. In each step we treat the datum as an integration of (1) all lower types (already isolated) and (2) the distinctive type which we are seeking and which marks the behavior off as an advance upon lower types. We first eliminate from our problem all which the lower types are adequate to explain. This may involve a good deal of observation, but is necessary to isolate the behavior which is as yet unexplained. We then observe the remaining behavior which is peculiar to the type we are studying, until we have marked the constant elements in the learning involved. This gives us the nature of this learning in exact terms. It furnishes us with the *pattern of the learning*, or as we say, its *structural type*.

This method, be it noted, appears to contain no arbitrary elements either in the *shaping of the problem* or its *analysis*. It receives its orientation from the fact of the integrity of the organism. It follows the serial order of organic advance. Throughout the analysis, it maintains emphasis upon function *pari passu* with that upon structure, thus holding to the *central problem* and holding to it in *exact terms*. In short, every step of the method is pertinent to the nature of the datum and problem, and the end-result is in exact and

definitive form. Results are therefore not relative to any partial viewpoint but to the whole field of animal learning for which they furnish *a backbone of exact truth.*

The inorganic method is the general method of science. It is the effective method for *piecemeal* studies of all sorts. It is capable of very wide application because of its discursive nature. For reasons we need not attempt to give, it is suited to the nature of intellect, and a great number of problems of organic science, as well as inorganic, can be set up according to its pattern. It has limitations, however, and to see them will only enhance its correct use as a method.

The inorganic method is inadequate for *central organic problems.* These problems do not represent a wide field, but they represent a very fundamental and important field. They require a method shaped to their distinctive nature as organic problems. For such problems, the *organismal method* described above is offered as an observational and exact method. In connection with the elemental method of inorganic science this organismal method should prove of value to all organic sciences. Its importance to psychology seems to be obvious. It is needed to give a basis of objectivity for the study of human psychology. It appears evident also that a sound organic method of analysis will furnish very fundamental guidance to social and philosophic thinking.

DISCUSSION

ONE SET OF POSTULATES FOR A BEHAVIORISTIC PSYCHOLOGY[1]

Why should the fundamental principles of psychology be reformulated?

Phenomenological Ambiguity

The categories, mind, consciousness, awareness, purpose, volition, sensation, image, feeling, etc., have failed to establish the degree of phenomenological specificity which is essential for a uniform program of scientific investigation. If a group of psychologists were asked to define, describe, and explain what they understand by the term consciousness or mental, the reports would show such divergences that a scientific quantification would be impossible. This lack of agreement is increasing rather than decreasing.

Dualism *versus* Monism

The antecedents of traditional psychological phenomena cannot be compared with, and causally related to, the ontogenetic, phylogenetic, and evolutionary antecedents of natural science phenomena. If we ask, what are the evolutionary antecedents of the mental states of redness, of sourness, of pleasantness, and then postulate some measureable physiological condition as a correlate to the mental state, we actually reduce mental evolution to physiological evolution, thus raising the question as to the scientific expediency of assuming a mental series at all. To explain human achievement we do not need *both* a mental and a physical system.

Mental Processes Are not the Antecedents of Social Achievement

The traditional psychological categories have not been integrated into the phenomenological series of events and objects which constitute human achievement and civilization as represented by the educational, vocational, administrative, recreational, and personal activities of the individual, or as represented by the corresponding social institutions. Even supposing it to be possible to secure a

[1] Presented before the Section of Psychology of the British Association for the Advancement of Science, Toronto, August, 1924.

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comprehensive record and description of the so-called mental states or mental processes of any given individual from birth to death, such a record is not a record of the antecedents of those actions nor of the social effects of those actions which represent individual or social achievement.

The Conditions for a Reformulation

I assume that a reformulation of the psychological postulates is justified if it achieves a greater measure of agreement as to their phenomenological limits; if it establishes a methodology which replaces the mind-body dualism by a systematic monism based on the assumptions of the physical sciences; and if the psychological elements that are adopted form the antecedents and components of individual conduct or behavior, and of social achievement or civilization. The problem of reformulation is not so much one of the accuracy of observation and measurement, as it is one of classification.

The Postulates

In the set of postulates which follow, human behavior and social achievement are assumed to be forms of motion. It is not expected that there will be complete agreement on the propositions which are given in support of these assumptions, but it is expected that the phenomenological limits to which these propositions refer can be more uniformly established than can correlative propositions referring to psychical or mental phenomena. In other words, it is easier to agree on the properties of an electron or the nature of motion, than on the properties of a mental state or process.

1. *Electrons and Protons*

The universe is the sum of the movements of its fundamental elements, the electrons and protons. The electron is defined as the smallest unit of negative electricity and the *mobile* constituent of the atom. The proton is defined as the smallest unit of positive electricity and the *stable* constituent of the atom.

2. *Direction and Rate*

If the electrons are regarded as negative in sign and the protons as positive in sign, (*a*) the *direction* of the movements between electrons and protons conforms to the proposition that unlike signed particles move toward each other, and like signed particles move away from each other; (*b*) the *rate* of motion being an inverse function of the squares of the distances between the particles.

3. *Stability and Form*

The operation of the laws of *direction* and *rate* exclude, so far as mathematical theory indicates, the possibility of utter condensation, or utter dispersion, or of permanent stability (zero motion) of all the particles. As a result, limited aggregates of electrons and protons form motion groups of varying symmetries, configurations, and degrees of stability, ranging from the relatively simple movements in the neutral hydrogen atom, made up of one electron and one proton, to that degree of geometrical complexity represented by such aggregates as the earth, planets, stars, and the objects and events occurring on them.

4. *Organization*

Electron-proton aggregates form various types or categories of interacting dynamic systems which under the present physical conditions on the earth are evolving into organizations of increasing complexity with respect to the number of electron-proton systems involved and with respect to the variety in the forms of the resultant movements.

Free electrons and protons evolve into the atomic type of organization, atoms evolve into the molecular type, molecules into the crystalline type, on the one hand, and into the protoplasmic type, on the other. The protoplasms evolve into the unicellular form of organization, and the single cells into the multicellular or organismic type. These in turn evolve into the *compound* multicellular or social type of organization. The totality of these dynamic electron-proton interactions forms the cosmos or the movement continuum.

5. *The Individual*

The individual is a locus in the movement continuum and the movements within this locus are correlational functions (in the mathematical sense) of all the movements that are occurring in the electron-proton organizations *not* within the locus. More specifically, the human individual is defined as a locus in the movement continuum, constituting a relatively permanent electron-proton aggregate (the atoms, molecules, and tissues of the body) interacting with the electron-proton systems not within the body, to form the series of energy interchanges designated as the life processes of nutrition, reproduction, and adjustment.

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6. *Behavior*

The sensori-motor and contractile effects which make up the language mechanism of the individual represents both a stimulus and a response system that exhibits a high degree of correlation with spatially and temporally remote environmental changes, and through which the movements of the individual become relatively independent of the non-language environment. The language mechanism forms the characteristic factor in human behavior.

7. *Group Behavior*

The totality of the language responses and the language records of a group approach as a limit the mathematical conception of unit functional correlation between the linguistic achievements of a group and all the changes that have occurred or are occurring in the movement continuum. In the individual a restricted sensory range, faulty inheritance, restrictive food and shelter activities, wasteful competition, disease, and death, limit this unit correlation to a finite value which is less than that of the group as a whole.

8. *Social Organization*

The language responses establish the compound multicellular or social type of organization in which there is sensori-motor interchangeability among individuals and within this social organization the individual approaches more closely the hypothetical condition of unlimited sensory range, perpetual youth, immunity against disease, the best inheritance, unlimited physical strength, and unfailing food and shelter resources. The organization as a whole exhibits such a complexity and variety of movements that it approaches more closely than does any individual in it, to a hypothetical limit which may be characterized as omnipotent, omnipresent, and omniscient.

9. *Civilization*

The sensori-motor interchanges among the individuals of a social organization develop institutions such as the industries, transportation, law, science, medicine, and education, which increase the variety in the behavior of the individual and reduce the limitations imposed by a restricted sensory range, faulty inheritance, restrictive food and shelter activities, disease, and death. Civilization is the cumulative effect of the behavior of the individuals in a group, toward achieving for the individual, unit correlation between

the totality of the electron-proton movements outside the locus of movements defined as the individual, and some function (such as the language responses) within the locus.

10. *Methodology*

The study of human behavior and human achievement is a study of the educational, vocational, administrative, recreational, and personal categories of movements. Any given adult activity is the terminal of two series of antecedents: (*a*) an ontogenetic series which traces backward through the sensori-motor modifications of the activity to some infantile form of movement; (*b*) a phylogenetic series which traces backward through the social or institutional modifications of the activity to some primitive social form. The *methods* of study are those of biology, anthropology, social statistics, and a special behavioristic method in which the individual's responses are analyzed into those physiological and social components which form the basis of those movements that establish the individual's social status in the group of which he is a member, and of those movements which establish the cultural or anthropological status of the group.

ALBERT P. WEISS

OHIO STATE UNIVERSITY